FROM AXIOMATIZATION TO GENERALIZATION OF SET THEORY

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ABSTRACT: The thesis examines the philosophical and foundational significance of Cohen's Independence results. A distinction is made between the mathematical and logical analyses of the "set" concept. It is argued that topos theory is the natural generalization of the mathematical theory of sets and is the appropriate foundational response to the problems raised by Cohen's results. The thesis is divided into three parts. The first is a discussion of the relationship between "informal" mathematical theories and their formal axiomatic realizations - this relationship being singularly problematic in the case of set theory. The second part deals with the development of the set concept within the mathematical approach. In particular Skolem's reformulation of Zermelo's notion of "definite properties". In the third part an account is given of the emergence and development of topos theory. Then the considerations of the first two parts are applied to demonstrate that the shift to topos theory, specifically in its guise of LST (local set theory), is the appropriate next step in the evolution of the concept of set, within the mathematical approach, in the light of the significance of Cohen's Independence results.

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FOR MY PARENTS.

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PART III: THE GENERALIZATION OF SET THEORY

NOTATION:

$\Pi x P(x)$	'for all x, P(x)'
$\Sigma x P(x)$	'exists an x, such that P(x)'
$\# \alpha$	'not α'

For sets X, Y:

$X \leq Y$	'X is a subset of Y'
$X \cup Y$	Union of X and Y
$X \Delta Y$	Intersection of X and Y
$f \cdot g$	Composition of the function f with g where cod(g)=dom(f)
$f \uparrow X$	Restriction of the function f to X
$\Gamma \vdash \alpha$	'there is a proof of α from Γ'
$\Gamma \models \alpha$	'α is a logical consequence of Γ'

INTRODUCTION

In 1963 Paul Cohen proved the independence of the Continuum Hypothesis and the Axiom of Choice from ZF set theory. A major task for contemporary philosophy of mathematics is to analyse the significance of Cohen's results, particularly for the nature and viability of set theory as a foundation for mathematics. To achieve his independence results Cohen developed the method of forcing. The forcing technique has proved to be an extremely powerful tool for providing independence results across a wide spectrum of mathematical disciplines. Many important open questions in, for example, topology, group theory and measure theory, i.e. questions generated by concerns within these disciplines rather than set theory, have been shown to depend on the underlying universe of sets presupposed. Thus the foundational import of Cohen's results permeates mathematics - a fact which underscores the importance of the aforementioned task. This thesis is intended as a contribution to this task.

Although in the mid-sixties several views on the foundations of mathematics were offered in response to the independence results, usually by philosophically minded mathematicians, subsequently, little philosophical work arising directly from these results was undertaken. Chief amongst the views put forward were the following:

a) we should adopt a formalistic attitude (Robinson 1964, Cohen 1971)

b) we should adopt a second-order axiomatization of set theory (Kreisel 1967)

c) we should adopt a constrained relativism i.e. adopt as a foundation the class of consistent extensions of ZF, e.g. ZF+CH, ZF+#CH (Mostowski 1967)

d) we should adopt an alternative to set theoretic foundations e.g. (Lawvere 1966, MacLane 1968, 1971)

My contention is that none of the above are satisfactory as a progressive response to the foundational challenge offered by the independence results. Rather, a combination of elements from c) and d) is needed. More specifically, we should: in essence retain a set theoretical foundation, adopt a relativistic view, but rather than Mostowski's version, employ the generalized set theory yielded by topos theory, i.e. local set theory.

In this thesis, then, I aim to establish that topos theory is the natural generalization of the mathematical theory of sets and is the appropriate foundational response to the issues raised by Cohen's results. The thesis comprises three parts. The first two parts derive from the following comment of Bernays on Mostowski's 1967:

> The first essential thing which emerges from his paper is that the results of Paul J. Cohen on the independence of the continuum hypothesis do not directly concern set theory itself, but rather the axiomatization of set theory; and not even Zermelo's original axiomatization, but a sharper axiomatization which allows of strict formalization. [1967 p.109]

In part I I discuss the relationship between 'informal' mathematical theories and their formal axiomatic realizations. In part II I turn to the transformation of Zermelo's axioms into ZF, in particular Skolem's reformulation of Zermelo's notion of 'definite properties', i.e. "the sharper axiomatization which allows of strict formalization". Parts I and II provide the appropriate context in which to determine the philosophical and foundational significance of Cohen's independence results. In part III after an account of the emergence and development of topos theory the considerations of the first two parts are applied to underpin the shift to topos theory.

PART I: PROBLEMS OF FORMALIZATION.

(1) It is important to analyse the relationship between informal and formal mathematics.

Amongst philosophers of mathematics there are to be found widely varying attitudes towards the relationship between formal systems and informal mathematics, the status of axioms, and indeed, the nature of informal mathematics. As examples we may cite the different opinions on these matters evinced by Frege and Hilbert. As well as differing views amongst philosophers of mathematics, there are often ambiguities, if not outright contradictions, within the work of individual commentators. Of greatest relevance to the relationship in question are the following two views:

i) the subject matter of the informal body of mathematics is understood to be a given realm of objects e.g. a universe of abstract sets;

ii) the formal system is construed as an implicit definition.

My aim is to show that these views face severe problems and in certain instances are untenable. It is important to stress, however, that I am not *directly* concerned with criticising, for example, commitment to realms of abstract mathematical objects, but rather the views concerning the relationship between such realms and formal systems. We will be able to identify one such view that, given my analysis of the genesis of ZF in Part II, is the most viable in the case of that theory. Moreover, it underlines my claim as to the appropriate foundational response to Cohen's independence results.

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Since the work of the 'founding fathers' Cantor, Frege, Russell, Zermelo and Skolem, we may recognise three clear landmarks for the three interrelated disciplines of foundational studies, mathematical logic and set theory. These are:

(I) Gödel's Completeness Theorem (1930),

(II) Gödel's Incompleteness Theorem (1931),

(III) Cohen's proof of the independence of the Continuum Hypothesis and the Axiom of Choice from Zermelo-Fraenkel Set Theory (1963).

Mathematical logic, set theory and the various foundational studies are highly developed mathematical disciplines in their own right; each generating internal programmes of growth and problem solving, though with an equally important web of relations between them. Set theory, in particular, may be taken as a pure mathematical theory apart from any consideration of its foundational role. However, it needs only a brief retracing of steps to see that the major importance of these disciplines is philosophical. It is only in the context of mathematical philosophy that the results (I), (II) and (III) may be reasonably judged to be landmarks.

The major results in these disciplines are not merely technical achievements. Their interpretation and analysis is a vital task for the philosophy of mathematics, particularly for contemporary philosophy of mathematics because of the high degree of formalization and axiomatization in twentieth century mathematics. Formalization is firmly entrenched within the methodology of contemporary mathematics and we need to examine whether formalization poses any special problems for a foundational discipline. An analysis of the relation between informal and formal mathematics is an indispensable component of this task. In this part of my thesis I discuss the relevant features of this relation. For it is the general topic of axiomatization and formalization of informal mathematics that is fundamental to the analysis of the foundational and philosophical significance of Cohen's independence results. More specifically, after laying some groundwork in chapter 2, I examine the difficulties following from what is initially an intuitively plausible account of this relation. But to begin with let us briefly look at the results (I), (II) and (III), indicating their significance for the issue of the relationship between formal and informal mathematics.

Let us construe our intuitive notion of classical first-order logical truth as "true in all structures". The Completeness Theorem for first-order predicate logic establishes that if a sentence is a first-order logical truth then it is derivable from the axioms of first-order predicate logic. In the context of formal metamathematics the generalized completeness theorem is equivalent to an algebraic result, namely that every filter in a Boolean algebra may be extended to an ultrafilter, so the strict formalist could take the completeness of first-order predicate logic as nothing more than a purely algebraic result. However, this would mask the fact, important for philosophy of mathematics, that in some sense we have established a "matching" of certain important semantic and syntactic notions. Or, more suggestively, that the formal system of logic has "captured" our intuitive concept of logical truth. I have said that a "matching" has been established "in some sense". (One issue here is that the notion of an "arbitrary structure" or, more generally, that of putative model is problematic.) Now it is part of the objectives of an analysis of the relationship between formal and informal mathematics to make this sense perspicuous.

Gödel's Incompleteness Theorem states that any first-order theory whose set of axioms is recursive and rich enough to contain first-order Peano arithmetic as a subtheory is either inconsistent or is incomplete in the sense that there is a true arithmetical sentence which is independent of the theory. Thus, in particular, first-order Peano arithmetic fails to capture all the truths of arithmetic. Historically, this theorem is important as it was seen to demonstrate the impossibility of fulfilling Hilbert's Programme. It is recognised that the theorem is much more than an historical curiosity, yet its full philosophical significance still remains elusive and very much a controversial issue. This issue essentially involves the relationship between a formal system i.e. first-order Peano arithmetic and some underlying informal or intuitive notion of natural number.

It is the relations of an informal notion of set to various possible codifications which underpins the philosophical significance of the independence of the Continuum Hypothesis from Zermelo-Fraenkel Set Theory (ZF). The following is roughly what I have in mind. Our starting point is an informal theory of sets which we consider sufficiently developed to decide what are taken to be basic questions, such as the cardinality of the continuum. The informal theory is then turned into a formal axiomatic theory. The next step is the recognition that the formal sentence which corresponds to the continuum hypothesis is not derivable, nor is its negation. The interest, here, arises from the understanding that the result informs us about the relation between our informal notion of set and the structure of the continuum. Have we found a weakness in the informal set theory or is our choice of formalism inadequate?

(2) Mathematical Intuition and Informal Mathematics

In analyzing the relationship between informal and formal mathematics it is important to consider the taxonomy of informal mathematics. Different sorts of informal mathematics may determine differences in what may be properly attributed to their relations to their putative formal realizations. However, the whole question of characterizing informal mathematics is extremely problematic and I shall not address it directly nor shall I offer a survey of extant views pertaining to the nature of informal mathematics and formalization other than the "snapshots" in the appendix. [see chapter (7)]. But what I believe is clear is that formalization, *in general*, is, and is recognised to be, a matter of degree. However, given the tools of contemporary mathematical logic and foundational studies a significant jump in the degree and character of formalization is evident and merits special investigation.

There is a distinction to be made between informal mathematics and intuitive mathematics. What I refer to as intuitive mathematics is, simply, mathematics that derives from some form of mathematical intuition. Intuitive mathematics is informal mathematics but only a proper part of informal mathematics. A full discussion of mathematical intuition, a notoriously difficult area, is beyond the scope of this chapter, but I will indicate the sort of thing I have in mind. According to Steiner

> ... there is evidence that the great mathematicians have been able to convince themselves of the truth of various mathematical propositions without knowing their proof. The Indian mathematician A. K. Ramanujan is perhaps the most striking example. Completely unlettered in

Western rigor, even later exposure to Western standards of proof did him no good, he had the ability to conjecture the most complicated formulas in the theory of elliptic functions, many of which were later proved. [1975] p.135]

The above passage suggests the following approximate characterization of mathematical intuition: **Mathematical Intuition is that mode of cognition through which we may come to recognise the truth of a mathematical proposition independently of any proof of that proposition.** This is clear enough for our present purpose. But, of course, the questions, "what constitutes a proof" and "when is a proposition a mathematical proposition" have by no means been settled.

When a body of informal mathematics is realized as a formal axiomatic system, or rather, when a given formal system is a putative formalization of a body of informal mathematics, certain propositions are distinguished in that their formal counterparts are taken to be the axioms of the system. My distinction between intuitive mathematics and the more general informal mathematics plays a part in locating a source of axioms. That it plays such a part is evident from my characterization of intuitive mathematics as independent of proof and is underlined below in the quotation from Gödel. [See also Zermelo on the axiom of choice in his 1904 p.141 and 1908 pp.186-7]

An important example of mathematical intuition is to be found within the philosophy of the epistemological platonist. Resnik explains as follows:

Let us call an *ontological platonist* someone who recognises the existence of numbers, sets, and the like as being on a par with ordinary objects and who does not attempt to reduce them to physical or subjective mental entities. An *epistemological platonist* is someone who also believes that our knowledge of mathematical objects is at least in part based upon a direct acquaintance with them, which is analogous to our perception of physical objects. [1980 p.162]

In particular, in the epistemological platonist view, we might come to know certain propositions of set theory via mathematical intuition. In Gödel's words:

Despite their remoteness from sense experience, we do have something like a perception also of the objects of set-theory, as is seen from the fact that the axioms force themselves upon us as being true. [1947 pp.483-484]

(3) Plausible Desiderata for Formalization

To provide a framework for a largely *general* discussion of the relationship between formal systems and informal mathematics I take a formal axiomatic system as consisting of three basic components; (F1) A language, and in this I include the syntactic categories, formation rules for the symbols and rules of inference; (F2) A set of axioms; (F3) The Semantics. The adjunction of (F3) is a necessary feature if we are to discuss the relationship between formal and informal mathematics. Without it, little sense could be made of any view construing the formal system as a formalization of a body of mathematics. However this component is to be understood quite generally. That is, as any means of interpreting the sentences of the formal system. In particular, there is no reason to confine ourselves to a formal semantics such as Tarski's for classical logic or Kripke's for intuitionistic logic.

Now suppose a given formal system is a formalization of some body of informal mathematics. It would seem plausible that the following two features are credited to that formal system. First, that it is a codification of the informal mathematics. Second, that it is a precisification of the informal mathematics. It is enough for my purpose to state these as plausible desiderata but it is a fact that the overwhelming majority of commentators on axiomatization and formalization accept these as obvious, if not necessary. Focusing on these two desiderata emphasizes the necessity of including (F3) in the characterization of a formal system - without it it there is no sense in asking whether the formal system has made the informal mathematics precise and certainly not whether it has codified the informal mathematics.

We could, of course, construe a formal system as only comprising (F1) and (F2). But in what sense could such a formal system be said to be a formalization of a body of informal mathematics? One answer is the following: at one point we started with an informal theory and rendered it into a formal system according to my characterization of formal system, i.e. with an associated semantics e.g. à la Tarski or perhaps some informal naturalistic interpretation, and subsequently to have discarded the semantics. But without some sort of semantics to begin with how could we even begin the process of codification?

Now once we have (F1), (F2) and (F3) we are at liberty to vary them. For example, if (F1) is a first-order language, we might change (F3), if appropriate, from a Tarskian to Kripkean style semantics. According to my characterization the result would constitute a new, *different* formal system. Further, a new judgement as to whether or not the resulting formal system constitutes a formalization of a given body of informal mathematics would be called for. In particular, whether the new formal system codified and precisified that mathematics. It is because I am interested in codification and precisification as required features of a formalization that I have included (F3) into my characterization. Without the adjunction of (F3) there is no sense in asking whether the formal system codified and precisified the informal mathematics. (F1) and (F2), then, constitute no more than a calculus of symbols and without a semantics of some sort adjoined to them cannot be evaluated in regard to their relations to a given body of informal mathematics in the relevant respects.

Having provided a general characterization of a formalization together with two plausible desiderata we may go on to specify certain constraints on the former and distinctions with respect to the latter that are relevant in the given context.

A distinction needs to be made between what I shall call *syntactic* precision and *semantic* precision. Consider the first-order theory of groups. The language of this theory, that is the items indicated in (F1), and also its axioms (F2) are given recursively. In other words, notions such as "axiom", "proof" etc. are effective and we may effectively perform logical operations on them, e.g. apply the rules of inference, concatenate proofs, etc. The precision of the first-order theory of groups is here judged with reference to the theory *qua* calculus of symbols. Syntactic precision, then, refers to the the formal system *qua* calculus of symbols and the degree of effectiveness of that calculus. This is a relatively unproblematic notion and may even to some extent be treated mathematically, for example, by the theory of Turing degrees.

Semantic precision is rather more difficult to characterize. Intuitively, it is the degree to which the formal system renders the informal mathematics clear and unambiguous and the degree to which it is "faithful to the meaning". The emphasis here is on the perspicuity of the notions of the semantic component relative to those of the informal mathematics. But as Wedberg points out

> The sentence-forms of a formalized language should have a meaning which is 'clear', 'sharp', 'precise', or 'exact'. What is meant by these words may be felt, but has not, to my knowledge, been satisfactorily clarified by anyone. If the meaning of a formalized language is given through the method of translation, the meaning will be of the same degree of clarity as the meaning of the language into which the translation is done. [1984 Vol. III p. 272]

We cannot, unlike in the case of syntactic precision, offer a mathematical characterization of semantic precision. At the same time, unless we forego interpretation of our formal systems and in particular eschew discussion of the relationship between informal and formal mathematics, in effect succumbing to formalism, semantic precision must be taken to be an important issue, especially so where we are formalizing a foundational discipline. (Incidentally, Russell recognised that this issue could cause problems for a foundational system such as *Principia Mathematica*. [See for example Russell 1923 and Rolf 1982])

As Lakatos has eloquently pointed out, in 'live' mathematics concepts are often in a process of modification or rigorization or both. Often the process of rigorisation, or rather putative rigorization, is facilitated by a reduction. For example, during the period referred to as the 'arithmetization of analysis', concepts such as 'continuity' and 'limit' were defined in terms of concepts like 'natural number' and 'set' etc. Plainly we should enquire as to the nature and implications of this process. In what sense have we transformed the original concept? In what sense have we 'captured' the original concept? Have we clarified the original concept, in particular, in the sense that the terms used in the definiens are more perspicuous than the definiendum? When we formalize an informal body of mathematics and then interpret the formalism, i.e. apply the semantics, the above considerations re-emerge. Lakatos, who in one form or another greatly concerned himself with these issues, posed the questions in the following manner: he first set up his target with the rather bold assertion that

> ...we should speak of formal systems only if they are formalizations of established informal theories...There is indeed no respectable formal theory which does not have in some way or another a respectable informal ancestor. [1978 p.62]

and after some discussion arrived at the position that

> Up to now no informal mathematical theory could escape being axiomatized. We mentioned that when a theory has been axiomatized, then any competent logician can formalize it. But that means that proofs in axiomatized theories can be submitted to a peremptory verification procedure, and this can be done in a foolproof, mechanical way. Does this mean that for instance if we prove Euler's theorem in Steenrod's and Eilenberg's fully formalized postulate system it is impossible to have any counter-example? Well, it is certain that we won't have any counter-example formalizable in the system (assumming the system is consistent); but we have no guarantee at all that our formal system contains the full empirical or quasi-empirical stuff in which we are really interested and with which we dealt in the informal theory. [Ibid., p.67. Note that Lakatos equates formalisation with effective formal systems i.e. Hilbert style systems.]

Like considerations are crucial for a foundational theory where we are attempting an across the board reduction of mathematics. For example, the logicists Russell and Whitehead undertook such a reduction, involving a wholesale redefinition of mathematical terms, in the ramified type theory of *Principia Mathematica*. This system, in the tradition of logicism, was a unified theory of logic and set theory. We may note here that a unified theory of logic and set theory, though not on behalf of logicism, has recently been urged by Mayberry [1977] in response to some of the problems we shall be considering below, in particular for the reason that such a system as he proposes is more in accord with Cantorianism. In fact, topos theory, is also such a unified theory. But it is Brouwerian rather than Russelian in the sense that the mathematics generates the logic, or in Bell's terms, the logic is synthesized from the mathematics.

Now to a certain extent the requirement of codification begs the question of semantic precision - at least in so far as we construe semantic precision as "faithful to the meaning". If, for instance, the semantics do not show the axioms to be "faithful to the meaning" of the informal mathematics then this might be a warrant for denying that the axioms codify that informal mathematics. In other words, it might be argued that we cannot realise *both* codification and precisification - since *by precisifying we are mutilating content and hence not codifying what was there in the first place*. However, my response is that this is a situation that cannot be judged in an absolute manner, an all or nothing affair so to speak. We might take as an analogy the choice that a translator is faced with. Several possibilities may present themselves as a correct *literal* translation of a given sentence while at the same time we may judge one translation to be better or more faithful than another.

If we accept that formal systems, at least in some instances, formalize *something*, i.e. that they may be emodiments of informal mathematics, then the question of semantic precision must be taken to be a serious and important consideration, albeit, an allusive one. The quotation from Wedberg suggests a criterion according to which an informal body of mathematics may be said to have been made semantically precise by a formal theory. We may state the criterion as follows: the formal system makes the informal mathematics precise if the semantic component (F3) is more perspicuous than the informal mathematics. In other words, the notions associated with the semantics are of a "greater degree of clarity" than those of the informal mathematics. Of course, this criterion is pitched at only a bare intuitive level and there is no suggestion that it is an effective method for deciding the question of precision. Apart from switching to an explicitly relative construal of precisification it simply states an intuitive requirement. Nevertheless, the criterion follows straightforwardly from the, admittedly rather approximate, account of semantic precision and will be useful in generating questions regarding the enterprise of formalization. Note that Wedberg confines himself to the case where we are "translating" i.e. moving from one language to another. My criterion is more general in that I allow the possibility that the informal body of mathematics need not be construed as a collection of propositions or in any way embodied in language. Take, for example, the "perceptions" of the epistemological platonist. It may be the case that certain sentences of the formal theory "force themselves" upon him as being true without the existence of a linguistic intermediary between the "perceptions" and the formal sentences.

It is helpful to make a further distinction regarding the semantic component of a formal theory. On the one hand there is the case where the semantics affords us an interpretation of the formal theory sentence by sentence, so to speak. On the other hand, there is the more holistic model theory. In the first case we are dealing with a single interpretation, e.g. as in Hilbert's reduction of Euclidean geometry to real number theory or, more generally, particular models in a set-theoretic sense - and this also includes prescriptions for interpreting sentences. These prescriptions may, for example, subsume some set-theory as is the case with a Tarskian style semantics.

As a particular case we might also interpret the formal sentences in the sense of the original informal mathematics. For example, suppose we understand ZF to be a formalization of the informal theory of *the* realm of sets in Platonist heaven. A formal sentence of ZF may then be interpreted in the sense of the informal set theory being formalized, e.g. the terms of the formal theory may refer to sets in the platonic realm of sets as opposed to say objects in a model. That is the term ω refers to the "real" set of finite ordinals. It may also be the case that an informal set theory may not incorporate an extensional view of properties, functions and relations. Thus, for example, a function symbol in the formal language might not be interpreted as a set of ordered pairs if we are interpreting the formal theory in the sense of the informal. As a matter of historical fact it was Hausdorff in 1914 who first produced a set theoretical reduction of the notions of function and relation i.e. some six years after the axiomatization of Zermelo. So in Zermelo's !908a, for example, functions and relations were not interpreted in terms of ordered pairs. [See Hallett 1984 p.265]

Suppose we are satisfied that we have codified a particular body of informal mathematics. Then the model theory provides a means of investigating the informal mathematics or 'exploring' its concepts. The model theory may make the informal theory precise in the sense that it delimits, for example, via independence results, the scope of the informal mathematical concepts. But here problems arise in that the model theory is itself embedded in another theory - usually a set theory. So for example, relative to ZFC certain important conjectures of group theory have been found to be independent. We may now ask whether these independence results inform us about our informal concept of a group or are merely theorems of ZFC with no bearing on our concept of group. Further, if we take the former attitude what do we make of the fact that some of these conjectures are decided by ZF set theory with the addition of the axiom of constructibility?

Having given my characterization of a formal axiomatic system and two desiderata for such a system to be considered a formalization of a body of informal mathematics the question whether these desiderata may be realized arises. If not, what implications does this have for our view of the nature and purpose of axioms, formal systems, and in particular the formalization of foundational disciplines?

There are a bewildering array of types of formal system currently in use. On the other hand, there are certain constraints on formal systems which have been, for a variety of reasons, commonly approved and as a matter fact systems so constrained have achieved, not without controversy, a preferred status within contemporary mathematics. Here I refer to effective classical first-order systems and I shall confine myself to this class of systems in addressing the above question. The constraints in question constitute the subject of the next chapter. [As an example of the increasingly evident shift beyond this class of systems in foundational studies see "Model-Theoretic Logics" edited by Barwise and Feferman 1985]

(4) Modern Mathematics and Effective Classical First-Order Systems.

The characterization of a formal axiomatic system I have given encompasses an endless array of available types of formal system. For example, we may employ second-order logic, various intuitionistic and constructive systems, free logics, infinitary logics, logics with non-standard quantifiers, ω-logics, modal logics and many-valued logics. As well as Tarskian semantics and Kripkean semantics there are infinite-game semantics, topological semantics and we might as well mention those who propose a substitutional interpretation of quantifiers. However,

> Many logicians would contend that there is no logic beyond first-order logic, in the sense that when one is forced to make all one's mathematical (extra-logical) assumptions explicit, these axioms can always be expressed in first-order logic, and that the informal notion of *provable* used in mathematics is made precise by the formal notion *provable in first-order logic.* Following a suggestion by Martin Davis, we refer to this view as ***Hilbert's Thesis.***
>
> The first part of Hilbert's Thesis, that all of Classical Mathematics is ultimately expressible in first-order logic, is supported by empirical evidence. It would indeed be revolutionary were someone able to introduce a new notion which was obviously part of logic. The second part of Hilbert's Thesis would seem to follow from the first-part and Gödel's Completeness Theorem. Thus Hilbert's Thesis is, to some extent, accepted by many mathematical logicians. [Jon Barwise "First-Order Logic" in "Handbook of Mathematical Logic" 1977 p.41]

Out of context this passage is perhaps slightly misleading in that it claims that "classical mathematics is ultimately expressible in first-order logic". What is meant here is that it is expressible in a system whose underlying logic is classical first-order logic. Given this modest clarification, we must also underline what is being claimed here, namely that each *proposition* of mathematics is first-order *expressible* - and, of course, this brings in the question of semantics. This claim, however, is to be distinguished from the assertion that for each body of mathematics, e.g. arithmetic, there is a first-order system whose theorems express all and only those propositions of that body of mathematics. If the former claim is unsupportable then we cannot accept first-order formalization as being sufficient for codification and precisification. The latter claim clearly incorporates the former but it is not so clear that we may consistently hold the former whilst denying the latter. Suppose we confine ourselves to first-order systems and we take the former claim as justified. May we now not collect up all the propositions of a particular body of mathematics whilst choosing a subcollection sufficient to generate this collection as our axioms or, indeed, take them all as axioms? Is this not plausible since we are allowing ourselves fragments of set theory in the metatheory? Well, this simply depends on what further constraints we impose on our notion of formalization.

It is the practically universal practice of contemporary mathematics to employ (at least "officially" or "in public") classical first-order systems. Thus it is of special interest whether this class of systems realizes, with respect to a given body of informal mathematics, the dual desiderata of codification and precisification.

In this connection there is a metamathematical result which needs to be taken into account, namely Lindström's Theorem. This apparently demonstrates that first-order logic is characterized by its 'weaknesses' or what is sometimes referred to as its 'diseases'. Lindström's Theorem states that first-order logic is the only logic closed under conjunction, negation, and the existential quantifier which satisfies the Compactness Theorem and Löwenheim-Skolem Theorem. Hodges' view is that

It happens that first-order languages are excellent for encoding finite combinatorial information (e.g. about finite sequences or syntax), but hopelessly bad at distinguishing one infinite cardinal or infinite ordering from another infinite cardinal or infinite ordering. This particular combination makes first-order model theory very rich in transfer arguments. For example, the whole of Abraham Robinson's nonstandard analysis ... is one vast transfer argument. The model theorist will not lightly give up a language which is as splendidly weak as the Upward and Downward Löwenheim-Skolem Theorems ... show first-order languages to be.

This is the setting into which Per Lindström's theorem came... He showed that any language which has as much coding power as first-order languages, but also the same weaknesses which have just been mentioned, must actually be a first-order language in the sense that each of its sentences has exactly the same models as some first-order sentences. [Hodges 1983 p.85]

Note that Lindström's theorem characterizes first-order logic in semantic terms, i.e. Compactness Theorem, Löwenheim-Skolem Theorems and reference to *all* models of sentences. Thus the theorem appeals to a significant amount of set theory and we may only interpret and appreciate the import of the theorem to the extent of our acquaintance with and apprehension of this fragment of set theory. Hodges goes on to conclude that

I think it is fair to say that all of modern mathematics can be encoded in set theory, but it has to be done locally and not all at once, and sometimes there is a perceptible loss of meaning in the encoding.

One naturally asks how much of the credit for this universality lies with first-order logic. Might a weaker logic suffice? The answer is unknown. [Ibid. p.87]

Over and above the widespread emphasis on first-order systems there is a general focus on those that are referred to by Church as 'logistic systems' i.e. roughly those systems where the metatheory is embedded in primitive recursive arithmetic. Church characterizes these in terms of the effectiveness of the following: (i) primitive symbols; (ii) well-formed formulas; (iii) axioms; (iv) rules of inference. Church explains that

The requirements of effectiveness are (of course) not meant in the sense that a structure which is analogous to a logistic system except that it fails to satisfy these requirements may not be useful for some purposes or that it is forbidden to consider such - but only that a structure of this kind is unsuitable for use or interpretation as a language. For, however indefinite or imprecisely fixed the common idea of a language may be, it is at least fundamental to it that a language shall serve the purpose of communication. And to the extent that requirements of effectiveness fail, the purpose of communication is defeated. [1956 p.52]

As illustration he states:

Consider, in particular, the situation which arises if the definition of well-formedness is non-effective. There is then no certain means by which, when an alleged expression of the language is uttered (spoken or written), say an asserted sentence, the auditor (hearer or reader) may determine whether it is well-formed, and thus whether any actual assertion has been made. [Ibid p.52-53]

Church's views on this matter are widely held and underwrite the emphasis on effective systems, though not, of course, the restriction to those that are classical or indeed to classical first-order systems. *That* particular restriction is underwritten by the considerations in the quotations from Barwise and Hodges. In

Fraenkel, Bar-Hillel and Levy 1973 we find some dissent from Church's contentions. There it is claimed that

> The arguments brought forward in Church ...to the effect that systems, whose rules of formation and transformation are non-effective, are not suitable for purposes of communication do not sound too convincing. Communication may be impaired by this non-effectivenes but it is not destroyed. Understanding a language is not an all-or-nothing affair. Our quite efficient use of natural languages shows that a sufficient degree of understanding can be obtained in spite of the fact that "meaningfullness", relative to a natural language is certainly not effective. [p. 286]

Whilst I would acknowledge the truth of the above passage, particularly in regard to natural language, it is misguided in that Church is addressing himself to formal languages, i.e. those constructed from what Parsons refers to as "artificial syntax" [see Parsons 1974 p.28], and with respect to these kinds of languages Church's contention is correct. With natural languages, which are in any case usually in a process of evolution, there is a vast web of social interactions which enable us to comprehend, to varying degrees, expressions displaying 'innovative' structure, e.g. modernistic poetry, the semi-gobbledygook of authority figures, jive talk, or just plainly bad grammar. But the comprehension of formal languages is, in general, strictly tied to formal semantics. For example, given the inductive prescription of Tarski-semantics for first-order languages, the process of interpretion simply breaks down at the point where we reach an ill-formed string of symbols. Note, however, that in saying that the interpretation of a formal language is tied to a formal semantics is not to deny that the formal semantics is itself embedded within natural discourse. The 'formality' of formal semantics may be said to reside rather in a combination of the *manner of* or *prescription for* application, that it is applied to formal languages and, to a greater or lesser extent, its artificiality. But from this it does not follow that the act of comprehension in the formal or informal case is itself any different.

Now if the notion of a formula is not effective and hence we are not always sure whether or not a string of symbols is (with respect to some semantic theory) asserting a proposition then it is doubtful whether we may even begin to address the question of codification. (Whether this is also the case where the notions of proof and axiomhood are not effective is less clear.) Note that in this connection we should also bring into relief the distinction between the *judgment* of whether or not a given system codifies a body of informal mathematics and whether or not *in fact* it does so. These are complex and delicate issues and I will take some of them up in the ensuing sections in connection with specific portions of mathematics - more often than not arithmetic and set theory. This will serve to clarify their import for codification and in particular the issue of whether the restriction to effective first-order systems is consistent with the demands of codification and precisification.

As a postscript I should like to make a further comment on the import of Lindström's Theorem. We have discussed the fact that effective classical first-order systems predominate in contemporary mathematics. Lindström's theorem emphasises that this logic is, in the given sense, *characterised* by Compactness and the Löwenheim-Skolem Theorems. This result underwrites the following argument. First-order logic came to be preferred, not because it is *the* 'right' or 'correct' logic, say, as opposed to second-order logic, or perhaps intuitionistic logic. (Most would grant that there are significant or genuine second-order logical truths yet second-order logic is not generally applied.) It is preferred very much for pragmatic reasons. More specifically, as is suggested in the above qoutation from Hodges, for the fruitfullness of its model theory. Undoubtedly the Compactness Theorem and the Löwenheim-Skolem theorems are the very foundations of contemporary model theory and thus a significant portion of modern mathematics. Furthermore, so far as preference of logics is concerned these two factors appear more important than Completeness.

(5) Can We Effectively Formalize and Realize Codification and Precisification?

Suppose we take the view that there exist objects to which the predicate 'positive integer' refers and that the statements of informal arithmetics are taken to be about these objects. In other words, the subject matter of informal arithmetic is a realm of objects, namely positive integers. Further, let us make the assumption that meaningful propositions with respect to the realm are definitely true or false. Now consider a classical first-order system with a recursive set of axioms that is a putative formalization of arithmetic. The first-order Peano-Dedekind axioms are an example of such a system. Now so far as codification is concerned this system falls short. Given Gödel's Incompleteness Theorem there are true sentences about positive integers that are theorems of the formal system. In other words the formalization has failed to codify the informal mathematics.

An attempt might be made to partition true arithmetical statements into those that are, as Paris and Harrington put it, " reasonably natural theorem[s] of finitary combinatorics" [1977 p.1134] or in Barwise's terms "strictly mathematical statements about natural numbers" [Footnote to Paris/Harrington 1977 p.1133] and those that are merely metamathematical pathologies with respect to arithmetic proper. Thus, so the argument might run, first-order Peano arithmetic is a codification of the "natural" part of arithmetic or rather the arithmetic of number theorists while Gödel sentences belong to the other part. Until quite recently this position had some credibility. However, this is no longer the case, for as Barwise relates:

> Since 1931, the year Gödel's Incompleteness Theorems were published, mathematicians have been looking for a strictly mathematical example of an incompleteness in first-order Peano arithmetic, one that is mathematically simple and interesting and does not require the numerical coding of notions from logic. The first such examples were found early in 1977,..... [1977 p.1133]

In general, in the light of Gödel's Incompleteness Theorem, if we have a body of informal mathematics construed as having as its subject matter some realm of objects, and we formalize it into an effective classical first-order system into which recursive arithmetic is encodable then this system will be incomplete and hence not realize the codification of the informal mathematics. Two well-known examples are the following:

(i) First-order Euclidean Geometry construed as the theory of points, lines, etc. and their relations in real space;

(ii) ZF-set theory construed as having its subject matter *the* platonic realm of sets.

Quite often, rather than referring to, say, the platonic realms of integers, sets, etc., the discussion is couched in terms of standard models. Standard models are set-theoretic structures and are, so to speak, the set-theoretic correlates of the realms in question. Some considerations about language apart, every sentence satisfied by the standard model is true of the respective realm and vice versa. The collection of sentences which are satisfied in the standard model is known as the 'theory' of the model. We may now characterize the failure of codification by stating that there is no recursive set of axioms whose closure is the theory of the model. Incidentally, I do not count the presence of non-isomorphic models as a failure of codification. If the closure of some recursive set of axioms *is* the theory of the standard model then we have full codification even if that system has non-isomorphic models. After all, the sentence 'the cat sat on the mat' has non-standard readings but that does not mean that we are warranted in claiming that its familiar use involves a shortfall of information.

Two responses to the failure of codification present themselves for consideration which do not require us to immediately withdraw from the position that our statements of the informal mathematics are objectively true or false statements referring to objects of some given realm. The first is suggested by the following passage from Parsons 1974

> Axiomatization is an especially thoroughgoing and rigorous instance of a process in the organization of knowledge that might be called systematization. [p. 26]

The first response, then, is that the axiom system codifies and makes precise the informal mathematics in so far as that informal mathematics represents a body of knowledge rather than some ideal complete collection of propositions or facts which, given the constraints, is in any case uncodifiable. Note here again the indispensibility of the (F3) component. Looking at the formal sentence it makes no sense to say we 'know' it, where the 'it' is divorced from an interpretation. This is even true of putative tautologies, where the propositional structure of the sentence rather than its complete interpretation is important; for we have to interpret the 'logical symbols' appropriately. The second response is the embracing of the fictionalist or conceptualist account of mathematics. Now whilst there is some initial plausibility in these two responses, particularly with respect to certain disciplines such as arithmetic and geometry for the former and set theory for the latter, in general they cannot be convincingly maintained given the imposed restraints. (However, the latter response affords the component of a solution or rather resolution, for the foundational problems of post-Cohen set theory, when conjoined to a critical analysis of the foundational role of axiomatic set theory and its development understood as the evolution of a concept.)

Construing a formal system as codifying and precisifying our knowledge of a given realm of objects raises the following problems:

(i) This view needs to be underwritten by a plausible account of mathematical knowledge and it is notoriously difficult to characterize or determine what constitutes mathematical knowledge. In so far as this view involves knowledge of abstract objects it has to counter forceful arguments, for example as in Benacerraf 1970 invoking the causal theory of knowledge, to the effect that we cannot have such knowledge.

(ii) Strictly speaking we are, in general, axiomatizing more than is 'known'. Let us take as an example first-order Peano Arithmetic. We may claim to know that the axioms are true of the realm of positive integers. We might know some of the theorems as self-evidently true, or let us say, as having the same epistemological status as the axioms, but are not included in the axioms since they are recognised as being derivable and we wish to 'streamline' the system. Some theorems may not be self-evidently true but are known by virtue of having been confirmed as theorems by actually being derived from the axioms i.e. we *possess* a proof. However, the vast majority of theorems, these constituting an infinite set of arbitrarily complex sentences, will not be part of what we know about positive integers, at least not known in the same sense that the axioms are known. But this is as it should be since part of the function of axiomatization is to prove hitherto unknown facts or conjectures about positive integers.

It might be argued that what is required is simply a distinction between what is known explicitly (e.g. the axioms) and known implicitly (the majority of theorems). But we have to make some sense of 'implicit knowledge' - for on the face of it it seems a queer sort of 'knowledge' that requires proof in order to find out what it is that one knows!

(iii) Suppose we identify the axioms as the codification and ignore the closure of the theory in this respect. But it is also problematic whether even the axioms are always to be included in what we *know* if they include schemas. What is it that we know when we formulate a system incorporating an infinite axiom schema such as the induction axioms of first-order Peano arithmetic? Do we know an infinite number of propositions which includes information involving arbitrarily complex predicates - or rather the interpretations of these predicates? In fact, it is more plausible to say here that we know the second-order form, knowledge which perhaps involves higher-order notions such as properties. In Kreisel's opinion

A moments reflection shows that the evidence of the first-order schema derives from the second-order axiom .... [1967 p.148]

But in this case we have to justify the priority of first-order systems and also the concentration on predicates rather than properties since the latter presumably are components of what we know. In short, this case prompts the question: "Why then are we axiomatizing within a first-order system?" The question is particularly embarassing if it is adjoined to an argument that the shift from the second-order axiom to the corresponding first-order schema involves a loss of content. For, after all, we are claiming to be axiomatizing what we *know* of, say, the positive integers.

(iv) The shift to the view that we are codifying our knowledge of a particular realm tends to make the informal mathematics difficult to identify in the sense that, for example, it is now no longer arithmetic *per* se we are dealing with but what is known of arithmetic and how do we decide what we know about positive integers? Moreover, when we say that a formal system constitutes a formalization of a body of knowledge does this mean a particular person's, school's, country's, professional community's knowledge or what? It even begins to get somewhat circular if we have this 'knowledge' in virtue of learning the axioms in a textbook or lecture. But don't many students learn set theory this way?

We might treat knowledge here in the manner that certain scientific theories are construed as knowledge, namely: highly confirmed general statements. There is a certain initial plausibility here in the case of arithmetic and geometry. (Gauss did go out and measure the interior angles of a triangle determined by three mountain peaks.) Though perhaps this strategy might be rendered plausible for arithmetic, since there is an infinite list in the induction schema and the axioms are claimed to be highly confirmed then these axioms are not all directly confirmed *individually* - they would have to be considered confirmed *en masse* i.e. we would have to argue the second-order version is highly confirmed. But is this strategy convincing for set theory, even though, for example, certain large cardinal axioms have consequences for natural numbers? What seems to be confirmed in the case of set theory, if not for that of arithmetic beyond the primitive recursive, is the consistency of the theory rather than its truth.

Let us look at the proposed strategy a little more carefully. The assumption is that the system codifies a body of knowledge about a realm of abstract objects (say positive integers) and the axioms are highly confirmed propositions. I have already mentioned the general problem about knowledge of abstract realms (cf. Benaceraff 1970) so let us turn to 'confirmation' in this context. The straightforward view here is that we are 'interpreting' the system in the real world by, for example, understanding the axioms as informing us on operations on finite collections of objects. So far we are only at best confirming consistency. To confirm truth we need an assumption of 'parallelism', i.e. structural similarity, between the abstract realm and the interpretation. But what warrant would we need for this assumption? The answer is - knowledge of the abstract realm - which is the very thing we are trying to confirm! Another point is that if the system in question is stronger than recursive arithmetic the interpretation would tend to be about 'ideal' operations and it is difficult to know how we could claim any significant degree of confirmation.

Without the doubtful assumption of parallelism, the strategy in question yields that the system is a theory about the world, i.e. yields an essentially empirical approach. This serves to undercut any *active* role that might be played by the realm of abstract objects which constituted our starting point. (As Gillies has pointed out, an Aristotelian account of the existence of abstract mathematical objects is compatible with an empiricist view of mathematics. So, for example, a given set of three apples i.e. the abstract object over and above its constituents is perceptible. On this basis the theory is interpreted in the real world (without invoking parallelism). But apart from any problems there may be about making sense of the use of 'perceptible' on this reading it is still not clear that the 'set' over and above its constituents is contributing anything as far as confirmation is concerned.)

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In the above I considered a response to the apparent failure of codification of which it was hoped that it did not place us in the position of withdrawing the presupposition that the statements of the informal mathematics are objectively true or false. As an alternative response I consider now the conceptualist or fictionalist account of mathematics. There are very many varied accounts of this view of mathematics. Principally, mathematics is construed as human activity with the emphasis on the imaginative or creative faculty. Consider, for example, the following view of Lakatos:

Mathematical activity is human activity. Certain aspects of this activity - as of any activity - can be studied by psychology, others by history...But mathematical activity produces mathematics. Mathematics - this product of human activity - 'alienates itself' from the human activity which has been producing it. It becomes a living, growing organism, that *acquires a certain autonomy* from the activity which has produced it; it developes its own autonomous laws of growth, its own dialectic. The genuine creative mathematician is just a personification, an incarnation of these laws which can only realise themselves in human action. Their incarnation, however, is rarely perfect. The activity of human mathematicians, as it appears in history, is only a fumbling realisation of the wonderful dialectic of mathematical ideas. But any mathematician, if he has talent, spark, genius, communicates with, feels the sweep of and obeys this dialectic of ideas. [1976 p.146]

Some accounts of the conceptualist view of mathematics are straightforwardly fictionalist, e.g. the mythological platonism described in Chihara 1973. There are some, however, which direct the mathematical imagination via the adjunction of more or less necessary constraints, for example, by application of Kantian doctrines (as in Hallett's unpublished "Gödel's Philosophy of Mathematics and Kant's notion of sensible intuition") or a phenomenological analysis (as in Tragesser's "Phenomenology and Logic" 1977). It is also appropriate to mention that Cantor, the founder of the mathematical theory of sets, also professed a brand of conceptualism. Cantor's conceptualism emerges in his well-known dictum that the nature of mathematics lies precisely in its freedom. [See Hallett 1984 on Cantor's "Free Mathematics" pp. 14-24]

It is true that as a response not requiring immediate withdrawal from the position that statements of the informal mathematics are objectively true or false and refer to objects of a given realm, conceptualism is questionable in that it may be seen to be severely weakening or at least distorting ontological commitments. Be that as it may, more direct issue can be taken with the claim of objectivity, though, in general, less so with Kantian or Phenomenological accounts than say with mythological platonism. In his account of mythological platonism Chihara suggests that

> ...one can hold that mathematicians construct their systems *as if* they were describing existing objects, *as if* there are such things as sets and numbers, and that he [*sic*] reasons accordingly. Whether such abstract objects exist, he can say, is irrelevant to the question of whether the mathematical theories are intelligible. It is enough that such objects can be conceived ...
>
> By a mythological platonist, I do not have in mind an irrational person who simply holds inconsistent beliefs or who is unwilling to accept the consequences of his own theories. A person studying a mathematical theory that asserts the existence of abstract entities need not commit himself to such entities: he could for example deny the existence of abstract entities and still remain consistent by remaining uncommited to the truth of the mathematical theory he is studying...[1973 p.62-3]

It seems then that a mathematician may, for instance, construct, or devise, two mutually inconsistent set theories as if each one were describing a realm of sets and at the same time deny the existence of either realm or in fact any realm of sets. We can take as an analogy two different descriptions of a character an author of fiction may have in different drafts of a novel (Melville's tale of the "The great pink-striped whale"?) In other words both realms are on a par in the sense that they are merely fictions. A fictionalist may 'explore' these imaginary worlds by elaborating his conception. This elaboration may either be purely deductive (that is, consists in drawing consequences from his pesent description) or ampliative, involving a further taxing of his imagination in producing essentially new descriptions. Let us call a *initial* description of such a realm a description that has not been augmented by the use of proof procedures.

Suppose we have, for example, two set theories describing two different realms of sets. Consider a proposition from the initial description of one of these realms and suppose its negation occurs in the initial description of the other. Are we entitled to claim that we recognise the *truth* of that mathematical proposition independently of any proof of that proposition? The question seems ill conceived since it is more appropriate to talk in terms of whether it is a correct or accurate description of the given realm. Suppose the proposition and its negation are both correct descriptions of their respective realms. If we insist here on the question of truth, then since both realms are on a par, we might be faced with having to deal with the prospect of concluding that they (the propositions) are both true. Since we are faced with asserting the truth of a proposition together with its negation we are entitled to question whether we have here effectively abandoned the notion of mathematical truth.

However, Chihara argues that mythological platonism does not entail the abandonment of mathematical truths but only that we must make the distinction between 'objectively true' and 'true to a concept'. But in the present context to say that a proposition is "true to a concept" is no more than another way of stating that it is a "correct description". What additional role does the invocation of truth play here? It seems that the mythological platonist is beginning to reveal himself as a closet formalist with a bad conscience and over-active imagination, particularly so if for Chihara "intelligibility" implies consistency and perhaps not much more. At least, as a general philosophy, it takes us no further than formalism in that it puts all consistent formal systems on a par.

Chihara's notion of 'true to a concept' clearly has affinities with the notion of 'truth in a model' in that a proposition true, for example, in one realm of sets may be false in another. But is Chihara's mythological platonist no more than a model theorist? The model theorist generally considers different models of his axioms, he is interested in the different models. More specifically, in the case where we have a concept such as group it is the collection of models and/or overall structural considerations that are to the fore rather than consideration of the individuals constituting the domain. [These attitudes are discussed in the following chapter on 'implicit definition'.] Now a realist doing model theory may wish to focus on certain 'standard models' - but unlike the mythological platonist he is committed to a realm of abstract objects. He certainly does not hold the 'as if' attitude referred to in the above quotation from

Chihara. From Chihara's characterization of mythological platonism there is no suggestion that there is an interest in the range of 'models' or 'realms'. The mythological platonist describes a *particular* realm or perhaps a limited number of realms. (In this weak sense he could be said to possess a standard model or models.) In the description of the mythological platonist true and false mean (as for the realist) true in the 'standard' model except he has more 'standard' models. Put bluntly, the mythological platonist is in precarious equilibrium between realism and the implicit definition approach.

But even if we take Chihara's distinction seriously, we can simply go on to distinguish between two notions of mathematical intuition. The first reading as before but "truth" taken in the sense of "objective truth". The second variety of mathematical intuition being characterized as: that mode of cognition through which we may come to recognize the truth *to a concept* of a mathematical proposition independently of any proof of that proposition. Whether the second of these notions is useful for the analysis of the relationship between formal and informal mathematics remains to be seen. But I judge it unlikely since there is nothing relevant prior to simply formulating the description.

Varieties of conceptualism, such as those discussed by Hallett and Tragesser, as opposed to Chihara's across the board thesis, tend to confine themselves to a particular concept (or family of concepts). In fact, in the given examples, the concept in question is that of 'set'.

So far as set theory is concerned there are strong reasons for adopting some form of the conceptualist thesis particularly in the wake of Cohen's results, though the Kantian or Phenomenological varieties tend to overstate the case. To begin with conceptualism provides a straightforward rationalization of independent propositions such as CH. Briefly, anticipating somewhat, what I have in mind is that the concept of set is viewed very much as an 'algebraic' concept together with a structuralist approach to the 'implicit definition' account of formalization. (Note that there is no structuralist thesis in Chihara's mythological platonism.) Moreover, this form of conceptualism emerges as the best option in the light of the historical development and treatment of set theory, coupled with its success *vis a vis* the reduction or formulation of mathematical concepts and its heuristic strength in the generation of new mathematical ideas.

Having now discussed two responses to the dramatic failure of codification evidenced by Gödel's Incompleteness Theorem, both, in their different ways, attempting to preserve the pressuposition of a realm of objects associated with the various bodies of informal mathematics, I turn now to a more radical departure, but not one without powerful proponents: Hilbert and von Neumann amongst them. This is the often misunderstood, and confused, account of formal systems as implicit definitions.

(6) Axiom Systems as Implicit Definitions

As Gorsky informs us

> When concrete meaningful axiomatic systems were constructed (c.f. the construction of geometry by Euclid), where the objects under consideration were regarded as given and to a certain degree analysed before the construction of the theory, and where explicit definitions of objects under study were introduced before formulating the axioms, the latter being interpreted as true propositions describing correlations between them, the question did not arise in the methodology of science whether axioms were definitions or not. Axioms were unconditionally excluded from definitions. [1974 p.40]

However, just before the turn of the century, underpinned by the development of set theory, and the influence of Hilbert, attitudes towards axiomatization began a process of radical alteration. This change is highlighted in what has become known as the Frege-Hilbert debate.

> The main point of disagreement between Frege and Hilbert was over the nature and purpose of axioms. According to Frege, axioms must be self-evident truths, thus the terms they use must be pre-equipped with meaning and denotation. Put bluntly, we should know what the axiomatized theory is a theory of. Hilbert on the other hand, took the view in his work on geometry and the real number system that axioms are "implicit definitions" of their terms, that is, they serve to pick out any system of objects that happen to satisfy them, and that this is all we need or have to say about points or numbers or whatever. One could be forgiven for thinking that the development of modern mathematics has completely vindicated Hilbert's view, for this "structuralist" approach .. has been dominant in twentieth century mathematics. Certainly structuralism has given mathematical research enormous freedom, since it appears to encourage the discovery and investigation of new structures through the modification or suppression of axioms of a given system. [Hallett: "Review of Michael Resnick: Frege and the Philosophy of Mathematics"]

The thesis, then, that axiom systems constitute implicit definitions amounts to this: that a 'natural number' or 'real number' is nothing more nor less than an element of a model of number theory or real number theory respectively. As Hallett puts it "they serve to pick out any system of objects that happen to satisfy them". There are various modified versions but this is the 'strong' version professed by Hilbert about the turn of the century particularly in connection wih his work on the axiomatization of geometry. To begin with I shall discuss this strong version. This will serve to bring the central characteristics of the thesis into bold relief and then I shall turn to a discussion of an amended form of the thesis which amongst other things is more in line with Hilbert's general approach and certainly more viable as a view of formalization.

Now, in fact, quite a few commentators have tended to shift to a stronger thesis in the sense that they shade over into outright formalism. This was so in von Neumann's case. As well as von Neumann quite a few workers in set theory at one time or another adopted the implicit definition view in some form, or at least professed, or appeared, to hold it. That Skolem understood Zermelo to have held this view in the 1908 axiomatization of set theory is evident from Skolem's first 'remark' in his 1922. That many who professed the view later shifted attitudes either explicitly or at least, as is usually the case, in effect, is not surprising given the highly unstable nature of the position. To demonstrate this characteristic of the thesis I shall adopt a set theoretical construal of model. Indeed, it is as good as written into the thesis. As such this is an acknowledgement that a set theory in some form or other forms a substantial part of the metatheory.

It is sometimes claimed that the implicit definition thesis makes no sense since, for example, we call ZF+CH and ZF+#CH 'set' theories, but they clearly have no models in common. Furthermore, how is it that we call ZF, NBG, NF, etc. set theories? Surely there must be a warrant to enable us to dub these various systems 'set' theories and thus there is a common notion that is not implicitly defined. But this is not an effective criticism. The term 'set', the thesis holder may retort, is merely a means of classification that owes its origins in some cases to a common core of axioms in others to no more than historical, psychological and/or accidental factors. It no more militates against the implicit definition thesis than does the fact that we have various group theories, e.g. abelian groups, non-abelian groups, etc and it is evident that group theory is no more than an implicit definition of 'groupies', if you like.

It is to be stressed that under the implicit definition thesis appeal to a standard model, unless it is picked out by pure convention, is no longer an option. Except in the case of a convention, the idea of a standard model is incoherent as an adjunct to the view of axioms as implicit definitions. There has been a suggestion that, at least for number theory, the anterior appeal to a standard model may be avoided if we employ a $L\omega_1,\omega$ language and apply Scott's theorem that, for countable model's $L\omega_1,\omega$-equivalence and isomorphism coincide. Thus we may claim a categoric number theory. But this only works if we sneak in a standard model of number theory into the metatheory. After all, the language allows countable conjunctions and disjunctions, i.e. indexed by the sequence of 'natural numbers' of the metatheory. But the structure of this index set, and in turn, the resulting class of isomorphic models, depends on the 'model' of number theory we are working with in the metatheory. [Cf. Weston 1976 on Kreisel's claims in his 1967 for a second-order formulation of set theory]

The mention of group theory above brings us to the following observation. The thesis that an axiom system constitutes an implicit definition begins to appear quite natural on an algebraic approach to mathematics. In part, this appearance is historically based. For example, the pioneers of modern abstract algebra e.g. Noether and van der Waarden were heavily influenced by the pre-formalist Hilbert even though they were working in the 20's and 30's. But apart from historical considerations it is plainly the case that an algebraic theory is not the theory of a *particular* realm. In the case of theories such as the first-order theory of groups we have many familiar structures that satisfy the axioms. These structures, if you like, are paradigmatic examples of the notion of group. But there is no question here of a standard model. We might 'discover' more about this concept by applications of the proof theory, or more likely, but controversially, applying a theory of models. However, we would not make a 'discovery' in the sense that we would feel obliged to add a further axiom. Moreover, the existence of propositions independent of the axioms as such is not problematic. Where with set theory or arithmetic we might expect, if by some lapse we were not taking the implicit definition thesis at face value, completeness or categoricity, it is quite the reverse for algebraic theories. It is part and parcel of algebraic practice to elaborate a given set of axioms. That is - at one time we may be particularly interested in abelian groups and at another in non-abelian groups.

Let us briefly look at the case of set theory. Here the problems inherent in the implicit definition thesis are brought into sharp relief. Suppose we consider the ZF-axioms as an implicit definition. A universe of sets is no more than a structure satisfying the axioms and the notion of set is relativized to one of these structures. But these structures are themselves sets and they cannot be sets in the sense just given. Are they sets in some absolute sense of sets? If so, then this is in conflict with the assumption for ZF of the implicit definition thesis. (Anyway, the thesis was proposed as a response to the failure of codification and any attempt at codifying this absolute set theory within an effective system is again bound to fail short.) Furthermore, the issue of codification aside, if we employ a system that has set theory incorporated into its semantics we will not be making set theory any more precise than our acquaintance with the notion of set in the semantics. One possibility is that perhaps we should also view these sets in some relative manner. In other words, we posit a never-ending hierarchy of universes of universes, etc. This seems unsatisfactory since it leaves us with the puzzling question as to what all these objects are. If we construe them as sitting in some Cantorian Super-Absolute then it seems we are doomed to begin yet another ascent through the hierarchies.

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In the discussion above on the thesis that a formal system constitutes an implicit definition reference has been made to 'structures', 'notion of a group', 'isomorphism' and 'categoricity'. But none of these figure in the characterization of the thesis. Furthermore I have tended to explicate the thesis in a rather indirect way using examples; specifically, number theory, group theory and set theory. So now I shall reconsider directly and in more general terms the thesis that a formal system constitutes an implicit definition. My purpose here is first to show that the thesis as it stands is untenable and then to discuss an associated thesis which explicitly incorporates reference to 'structure' etc.

The strong implicit definition thesis is that a system of axioms determine a class of interpretations, namely those that satisfy the axioms. Note then that an implicit definition is not a definition at all. The axioms of geometry do not define 'point', 'line' etc. However, axioms may be converted into a predicate within set theory which defines a class. For example, the group axioms may be formulated as a set theoretical predicate which holds of a set only in case it is a model satisfying the group axioms. But this is all entirely *explicit*.

The implicit definition thesis was here introduced as a response to the failure of codification. So, given a formal system, a natural question is, what is the informal theory of which the system is a putative formalization and does the system codify and precisify the informal mathematics? To answer this we must first be clear that in the case of the implicit definition thesis it cannot be a matter of whether the system is related to a particular realm in some way. For example, whether the first order Peano axiom's or ZF exhibit a certain relation to some realm of natural numbers or sets. According to the thesis a natural number or set is nothing more than an element in a model of the respective systems. Second, it is straightforwardly the case that the relevant semantics involved here, i.e. the (F3) component of the system, is a model theory.

The considerations in the paragraph above leads to the conclusion that if a formal system is an implicit definition in the strong sense then there is no informal theory of which the system is a formalization. So for example, if a natural number is nothing but an element of a model of the first order Peano axioms then this dispenses of a realm of objects whose informal mathematics is putatively codified and precisified by the formal system. Basically, we have an axiom system in a given language and its models and nothing preceding it of which the system is a formalization. In the case of group theory this position, at least at first sight, is not unappealing since the interest lies in the models. This is generally true of algebraic theories.

On the other hand the history of group theory informs us that there was an informal idea of a group. An informal notion of group to which we may refer in our judgments whether a formal system is a formalization of the notion of a group. I have already mentioned the existence of paradigmatic realizations and in fact it is clear that there have been sharp specific heuristics that have contributed to the formulation of the axioms, for example, the positive integers under the operation of addition. The informal mathematics is now becoming identified. For the moment I take it to be the informal notion a group. However, I am not claiming that there is a prior informal notion for all algebraic varieties. It is possible for a variety to be formulated by a random selection of axioms. Furthermore, once a concept such as 'non-commutativity' is noted for groups and say the abelian axiom is added to the basic group axioms then the idea can be considered generally in connection with other algebraic varieties. It becomes a familiar concept - and its application a known strategy - within algebra.

The first proposed modification to the strong implicit definition thesis is that whilst axioms do serve to pick out a class of models at the same time they may be formalizations of informal notions as in the case of group theory. As such the issues of codification and precisification are operative. The second modification is to make the thesis an explicit doctrine on structure. As stated the strong version makes no direct reference to the notion of structure. Of course, a model is a 'structure' and the axioms pick out those models with certain structural features but it has to be made explicit that the model theory which in Goodman's words "is the study of the semantic content of mathematical theories" is not essentially concerned with the 'points' of a given model. No doubt Hilbert and the Hilbert school of algebraicists supported this view. [See part III, Chapter 3.] Thus this modification underpins the references to 'isomorphism' and 'categoricity'.

I will now refer to this modified view of the status of formal systems as simply the implicit definition thesis. Now given this thesis where we have concocted an arbitrary set of axioms the issue of codification is trivialized. The non-trivial cases are those where we have a prior informal notion. These are the important cases so far as mathematical practice is concerned. [Cf. the quotation from Lakatos 1978 in chapter 3 p.-] But here there is no clear cut set of necessary and sufficient conditions determining codification. If anything it is a matter of judgement by the mathematical community and varies over time. The situation is analogous to the adoption by physicists of, for example, a particular definition of 'mass'. Note, however, that an axiom is not a true or false statement about a particular realm but is more akin to being a correct assertion about an informal concept. As a straightforward example, suppose we are considering the first order formalization of commutative groups where the (F3) component is a classical model theory. Then the formal sentence '$\Pi x \Pi y(x*y=y*x)$' may be informally understood (as opposed to its interpretation in a given application of the semantic theory) as prescribing that 'the binary operation on a group commutes' and thus judged to be part of the putative codification of the informal notion. At this point I shall make four more remarks on codification and the implicit definition thesis.

i) A necessary condition determining codification is that the paradigmatic structures associated with the informal notion be included in the collection of models determined by the formal system. Or rather, that their set model correlates be so included.

ii) The development of a mathematical concept is an evolutionary process including modification of a concept as a response to the emergence of important open problems. Furthermore, the problem of, and a given solution to, the formalization of an informal notion itself may serve to initiate a further stage in the development of the informal notion. That is, there is a feedback process between informal notions and formalizations. An important example is the axiomatisation and formalization of Cantorian set theory which will be discussed in this connection in part II.

iii) So far as 'group', 'ring', 'vector space', 'module' etc., i.e. paradigms of algebraic notions, are concerned, the implicit definition thesis is a sound viewpoint. But the thesis also covers the formalization of number theory, geometry and set theory. So ZF, for example, is considered as a putative formalization of an informal notion of 'set' picking out a class of structures. Put bluntly, an algebraic attitude is adopted towards these theories; they are treated just as the notions of group, ring, etc. On the other hand this is not to deny that they may have a special foundational importance or application. Note, that just as for the strong version, for the proponent of the implicit definition thesis the incompleteness theorem does not signal a failure of codification. (At the same time the thesis is not an *argument* against those who profess a realist attitude to say natural numbers or sets. If such a realist was willing to give up Hilbert's thesis then he could claim that the relevant reality was not completely axiomatizable in an effective first order system. This is also a coherent attitude for those professing a realist attitude towards the formalization of the laws of nature. However, in the mathematical case the problem of how we know what we know is not so straightforward.)

iv) Although I have introduced the implicit definition thesis as a response to the failure of codification, historically it was professed if not for arithmetic (or recursive arithmetic) then for geometry, set theory and abstract algebraic theories before the proof of the incompleteness theorem.

In construing classical first order systems as implicit definitions it becomes especially problematic as to whether we have made the underlying informal notions semantically precise. Reference to a standard model is no longer a legitimate option. Rather, we must refer to a class of structures. Notions such as group and natural number are reduced to set theoretic notions - a group is realized as a set theoretic structure and a natural number a set theoretic construction - and it is problematic whether these set theoretic notions are any more perspicuous than the original informal notions of group or natural number or whatever. An historical setting for the above is provided by Goodman. He informs us that

In the eighteenth century, mathematics was considered a science distinguished from the other sciences only in being more certain and more fundamental. Its special province was the laws governing space and quantity. In the course of the nineteenth century, this conception of the nature of mathematics was strongly undermined. First the non-Euclidean geometries were used to deny existence of a unique spatial structure for our intuitions to be about. Then analytic geometry was used to undercut the view that there was an intuition of space at all apart from our intuition of the numerical continuum. The end product of this development is the contemporary mathematician who tells his undergraduate students that by three-dimensional Euclidean space he *means* the set of all ordered triples of real numbers. Obviously, that is not what Euclid meant. Toward the end of the nineteenth century, even the intuitive conception of quantity or magnitude was replaced, at least officially, by the purely conceptual structures introduced by Weierstrass, Dedekind and Cantor. Again, a contemporary mathematician is likely to tell his students that by a real number he *means* a Dedekind cut. Obviously, that is not what Euler meant.

One effect of these changes was to produce what might be called a foundational vacuum - a situation in which mathematicians were without any sytematic account of the nature of the structures they were dealing with. Axiomatic set theory rushed in to fill this void. [1979 p.549]

The picture now presented by the implicit definition thesis is that a formal system identifies a class of models of the axioms and (with the exceptions referred to above) is a putative formalization of an informal notion. The semantics is a model theory i.e. a fragment of set theory. The investigation of the notion associated with the formal system is facilitated by this set theory. We can ask 'global' questions such as whether two abelian groups of the same cardinality of order p (a prime) are isomorphic or consider the Whitehead problem: "Suppose G is an abelian group with the property that whenever H is an abelian group extending the group Z, of integers, such that $H/Z \simeq G$, then $H \simeq Z \oplus G$ (direct sum). Is G necessarily free?" [As stated in Devlin 1979 p.150] However, the answers to these 'global' questions depend on the underlying set theory. A given question may be answered affirmatively, negatively or be independent depending on the underlying set theory. The underlying set theory provides construction principles such as e.g. axiom of replacement and key elements of the ontology e.g. the existence of a measurable cardinal. For example, if the underlying set theory incorporates the principle of constructibility then the Whitehead question is answered in the affirmative. In this sense the underlying set theory provides an arena or 'site' for mathematical activity and the realization of mathematical concepts.

The site delimits the notion of a group in that a variation in the underlying set theory in general brings about a variation in the class of groups and general propositions about groups obtained by application of the model theory. Furthermore, there is nothing in all this to suggest that there is anything amiss with the codification of the informal notion. In effect, the notion of 'group' has been relativized. Under the implicit definition thesis it is evident that the situation described above in the case of group theory holds not only for common algebraic notions but also for arithmetic, the theory of the continuum, geometry and set theory. In short, mathematical notions are treated algebraically and this approach accommodates relativism in a very natural way.

The underlying set theory may itself be formalized and since the implicit definition thesis applies generally it applies to this formal system also. A model of this system also constitutes a site of mathematical activity in that the considerations of the last two paragraphs may be interpreted as relativized to this model. That is, we consider the models as sitting in this 'universe of sets'. More generally we can construe a site of mathematical activity as a model of a foundational theory, e.g. ZF, and I shall employ the term in this general sense. The 'global' questions are not global in any absolute sense. They refer to 'all' models of a given system where the quantification is localized to a site. This is in keeping with the implicit definition thesis which as we have stated does not take formalization to be a process of codifying truths about some absolute realm of mathematical objects. In any case, as Mayberry has emphasized in his 1977 classical first-order logic is constrained in it's ability to codify any absolute sense of 'all' i.e. genuine global quantification.

As indicated in the quotation from Hallett at the beginning of this chapter the implicit definition thesis is associated with a general "structuralist" approach. The formal system identifies a 'category' in a naive sense in that notions such as structure preserving functions between models and categoricity are emphasised and (in practice) there is an identification of isomorphic structures. This was natural for the abstract algebraist but the thesis is in fact applied throughout mathematics. But these considerations are in anticipation of part III. There I shall discuss the emergence of category theory out of this naive context and how wherein category theory concepts were developed that unify, simplify and generally rationalize the structural viewpoint. However, as we shall see, this development does not lead to an eschewal of a set theoretical foundation.

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ZF and the axiomatizations and formalizations of set theory which are its precursors are putative axiomatizations and formalizations of an informal body of mathematics, roughly speaking, the Cantorian notion of set. But underlying these formalizations, although generally centred on the Cantorian notion, we are not faced with a fixed concept of set but rather with an notion in the process of evolution. It is clear from a critical history from Cantor to Cohen and post-Cohen developments that the notion has undergone significant transformations, particularly important being the shift from Zermelo's axioms to Skolem's. This last constitutes the subject matter of Part II.

(7) Appendix to section (2)

In Brouwer's philosophy, mathematics derives from intuition in so far as it is based on the Kantian doctrine of "Inner sense" which is the intuition "by which we are aware of our own states of mind in time." [Paton 1936 p.99] In so far as it carries with it an epistemological component, Kantian "Inner sense" accords with my reading of mathematical intuition. At times Brouwer, appears to confirm this view. Writing in 1907 he claimed that

> The primordial intuition of mathematics and every intellectual activity is the substratum of all observations of change when divested of all quality. [Ph.D Thesis p.8]

However,

> Mathematics is, according to Brouwer, not a *theory*, a system of rules and statements, but a certain fundamental part of *human activity*,... [Fraenkel, Bar-Hillel, Levy 1973 p.220]

At least as far as mathematics is concerned this activity primarily concerns itself with the ontology of mathematics. Inner sense provides the raw material for this activity. The existence of mathematical objects derives from iterated operations or "constructions" in the inner sense and previously constructed objects. In fact

> The fundamental thesis of intuitionism in almost all its variants says that *existence in mathematics coincides with constructibility*. [Ibid p.220.]

The emphasis in Brouwer's philosophy of mathematics is upon mathematical activity and in particular mathematical existence. What then about the status of mathematical propositions? May we , via inner sense, accept the truth of a mathematical proposition independently of any proof of that proposition? This is apparently an extremely difficult question. Körner answers in the affirmative. In fact, according to Körner, for the Brouwerian, this is the case for all mathematical propositions. He interprets mathematical theorems as reports on intuitive constructions and claims that

For the intuitionist every true mathematical statement is justified by a construction which is (i) a self-evident experience and (ii) not external perception. [1960 p.136]

On the other hand, in Fraenkel, Bar-Hillel and Levy it is emphasized that

The construction itself constitutes the proof... [1973 p.225]

However, the notion of proof here is rather weak since they also state

...one should drop the usual idea as though the demonstration were intended to convince the reader of the soundness of an argumentation by basing it, step after step, on recognized principles ... [Ibid.p.225]

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The role of intuition in empiricist accounts of mathematics is not altogether clear in those versions which include the use of a principle of induction. That is, either the mathematical principle of induction or the more usual inductive inference referred to in discussions of empiricism. Poincaré, whose philosophy of mathematics, according to Chihara, was in many respects empiricist,

> ... followed Kant in holding that there are some things we know through intuition, as for example that mathematical induction is a valid inference form ... [1973 p.145]

It follows that if the other postulates of Peano arithmetic, e.g. that no two positive integers have the same successor, are known intuitively, then Peano's postulates are intuitive mathematics according to my characterization. Note that it is likely that the majority of the consequences of these postulates may be considered part of the informal theory of arithmetic but not part of intuitive mathematics. They may be simply too complex or indeed be "surprising" results. This point also highlights the role of intuitive mathematics in indicating what are to be taken as axioms. (Consider the axiom of choice, i.e. Cantor, Ramsey, von Neumann versus Weyl, Borel, Lebesgue, and the continuum hypothesis in connection to the above.) It may also be the case that some propositions known intuitively are not added to the postulates since they are seen to be derivable from them. However, it might be argued that some of these postulates are "proved" by inductive procedures (i.e. those commonly discussed in philosophy of science). The arch-empiricist Mill took induction, in this sense, to be an inference of syllogistic form in which the major premise is suppressed. The major premise in question being an assertion of the uniformity of nature. In this case, if all the postulates are derived by induction then we have here a candidate for an intuition-free arithmetic. Kitcher, however, in his recent revival of a thoroughgoing empiricist philosophy of mathematics takes the view that

> Some mathematical statements are asserted on the basis of inductive or analogical arguments. Such arguments standardly will not fulfill

either of the functions of proofs, but they may nonetheless be used to warrant mathematical belief. [1983 p.181]

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I shall devote the remainder of this section to first drawing attention to an important distinction and second, in making some observations about informal mathematics and language. This also affords us a glance into some attributes regarding the transformation of informal theories into formal axiomatic systems.

The distinction is that between axiomatization and formalization. So, for example, as will be made evident, the systems of Peano 1889, Zermelo 1908a and Skolem 1922, though axiomatizations, in contradistinction to ZF, were not formalizations. Charles Parsons writes that

Axiomatization is an especially thoroughgoing and rigorous instance of a process in the organization of knowledge that might be called systemization. The objective is to organize a body of knowledge (or of a theory that aspires to be knowledge) in such a way as to clarify its structure and strengthen its justification *as a whole*. In particular, one seeks to single out certain concepts and principles as "primitive" or "fundamental" and others as "defined" or "derived". The method of axiomatization, first applied to geometry in ancient times and epitomized by Euclid's *Elements*, presents a theory by singling out certain primitive notions and defining others from them, and singling out certain propositions as *axioms* and deriving all other propositions of the theory by *deduction*.

These notions of definition and deduction are not without ambiguity. It is a mark of an informal axiomatic theory that a general background is used in developing it that is not itself axiomatized in the theory itself. In modern mathematics this background can include logic, arithmetic, and even some analysis and set theory. [1974 p.27]

In the above, Parsons states that in axiomatizing we "organize a body of knowledge". But if, for example, we axiomatize the theory of abelian groups are we entitled to claim that we have organized a body of knowledge - especially if we take seriously the contention that the axioms constitute an implicit definition? (Well, perhaps, in the sense that we know that much about abelian groups.) Note also that Parsons is slightly puzzling in the latter part of the quotation in that the informal axiomatized theory might be logic and this logic is also the background theory. This point may also hold for set theory.

For Parsons, after axiomatization, or in his words "rigorous axiomatization", two further steps yield a formalization, first: "all background theory, incuding logic, and everything given by the meaning of the primitives, are to be taken up into the axiomatization"; second: "we replace the language involved by an artificial syntax".

The impression given by Parsons by his use of phrases such as "singling out primitive notions and defining others from them" and "replace the language involved" is that informal mathematics, which is for him a "body of knowledge", is a collection of propositions, where the term "proposition" is interpreted as a linguistic entity, i.e. sentence. On that view a formalization involves a change of language and, moreover, the analysis of the relationship between informal and formal mathematics essentially involves the study of a particular relationship between two collections of linguistic entities. One a collection of propositions the other a collection of sentences of an artificial language. But is this straightforwardly the case?

It is an open question whether the "perceptions" of the epistemological platonist or "conceptions" of the mythological platonist are in a linguistic mode or in some form more akin to the sensual, e.g. visual or apprehended in the mind's eye, so to speak. Is the move from informal to formal mathematics analogous, say, to a logic student rendering a sequence of sentences of a natural language into the language of a first-order predicate calculus? Except that perhaps it might be argued that in the case of mathematics we need to retain content rather than just logical form. Or might it be, in some instances, akin to describing a landscape.

The Brouwerian takes an extreme view with respect to the connections between language and mathematics. The view is that mathematics is certainly a non-linguistic mental activity. In the Brouwerian philosophy

> ... the weakness of mathematical language in comparison with mental construction is stressed, for any language is, says Brouwer, vague and prone to misunderstanding, even symbolic language (since mathematical and logical symbols rest on ordinary language for their interpretation). Hence mathematical language is ambiguous and defective; mathematical thought, while strict and uniform in itself, becomes subject to obscurity and error when transferred from one person to another by means of speaking or writing. Thus it therefore would be a fundamental mistake to analyse mathematical language instead of mathematical thought. [Fraenkel, Bar-Hillel, Levy 1973 p.224)]

We might argue that to that activity of which we may impute error and vagueness we must allow even if only by sheer chance luck the possibility of correctness and clarity. But for Brouwer language is *essentially* vague. This does not necessarily vitiate the analysis of the relationship between informal and formal mathematics. However, Brouwer's view on the status of formalization does. Beth informs us that

> ... Brouwer gives a striking description of the successive stages in the formalisation of mathematics. He enumerates: (i) the construction of intuitive systems of mathematical entities, (ij) the verbal parallel of mathematical thinking, that is mathematical language; (iij) the mathematical analysis of this language; this activity leads to the discovery of verbal edifices established in accordance with the principles of logic; (iv) the step of abstracting from the meaning of the elements which constitute these verbal edifices; the abstract systems thus obtained are considered to be mathematical systems of the second order; they are identical with the formal systems studied by symbolic logic; (v) the introduction of the language of symbolic logic which accompanies logical constructions; this stage is found in the works of Peano and Russell; (vi) the mathematical analysis of the language of logicians; this stage, initiated by Hilbert, had been neglected by Peano and Russell; (vij) the step of abstracting ... etc. - According to Brouwer, mathematics is only to be found in the first stage of the process; the second stage is unavoidable from a practical point of view; the later stages are of a derivative character. [1959 p.411]

Thus, for Brouwer, there is no legitimate formal mathematics as such and hence there is no relation to analyse between formal and informal mathematics. The stage (iij) indicates a relationship between formal and informal languages but the Brouwerian claim is that this is far removed from the arena of mathematics. However, in stark contrast to Brouwer, some philosophers of mathematics working in the general tradition following on from Brouwer's intuitionism, in particular those who take as their starting point that mathematics is grounded in constructions, conceive mathematical activity and language as intimately connected. They also take a more positive view of the relationship between the informal mathematics and its formalization. For example, Machover confesses

> I strongly believe that *all* mathematics *starts* from construction, *starts* as schematic constructive activity. [1983 p.10]

It is the schematic nature of the constructions which contributes the linguistic component. Machover writes

> Let us start from a certain type of schematic construction. At this stage each proposition is a statement asserting the feasibility of such-and-such a construction. A proof of such a proposition consists in showing *how* to perform a construction of the kind the proposition claims to be feasible, and showing *that* the described construction is indeed of the required kind. These propositions become at least partly formalised. (All mathematics becomes at least partly formalised, if only in a fragment of a natural language. This seems to be one of the rules of the game; perhaps it has to do with the schematic nature of the constructions.)
>
> The fact that we are still at a constructive stage means that the logic which governs our discourse is constructivist logic (intuitionist logic). Though our mathematics is already formalised - partly or even completely - it is *not* meaningless. On the contrary, formalisation is merely a tool of precision. The postulates which we use are by no means arbitrary strings of symbols, neither are they implicit definitions of hypothetical entities. They are postulates in the old traditional sense : self evident truths about the constructions we are dealing with. Consistency is guaranteed *provided* we have managed to capture correctly in our intuition certain basic facts about these constructions. [Ibid. p.10]

Note that Machover's notion of formalization seems less constrained than Parson's since formalization does not presuppose an artificial language. He claims that formalization is *merely* a tool of precision. It is clear from the passage quoted above that this is not only syntactic precision. He also tends to give the impression that there is a stable underlying concept of construction and that the successive steps of formalization are to be construed as sharper and sharper characterizations of this concept. As a matter of fact Brouwer claimed that there was an absolute concept of construction but, as is argued by Tait [1983], Brouwer's account of it is inadequate. It has transpired that the conceptual analysis of 'construction' is proving to be extremely difficult. The fact that there are so many different schools of Constructivist Mathematical Philosophy is partly a reflection of the elusiveness of the notion of 'construction'. Curry, in the following passage, indicates a further aspect of its elusive character. He informs us that

> About 1930 Heyting gave a formalization of arithmetic which was compatible with intuitionism; somewhat later Gödel showed that classical arithmetic could be interpreted in intuitionistic arithmetic; this gives an intuitionistic proof of the consistency of classical arithmetic, whereas a strictly finitary proof would contradict one of Gödel's incompleteness theorems. Thus there is, from the finitary standpoint, a nonconstructive element in the intuitionistic arithmetic; just where this enters I do not know. [1963 p.15]

The significance of the aforementioned result, namely that: classical arithmetic is consistent relative to intuitionistic arithmetic is again embedded in the issue of the relationship between formal and informal mathematics. Does it provide evidence of the instability or incoherence of the notion of 'construction' or have we found a plausibility argument for the consistency of classical arithmetic? Consideration of these types of aspects of formal results brings us back to a central problem which constituted our starting point namely: the question of the philosophical and foundational significance of mathematical results.

PART II: ZERMELO, SKOLEM AND DEFINITE PROPERTIES.

INTRODUCTION

Cohen's independence results are metamathematical results. That is, they are theorems about a specific formal system, namely ZF, treated as a mathematical object. But their foundational and philosophical significance is to be located within the study of the relationship between a body of informal mathematics and its putative formalizations. So the following question arises: "What is the the informal body of mathematics of which ZF is a putative formalization?" In the wake of this question are the general considerations discussed in Part I. Clearly any given answer to the general problems raised in that discussion will affect attitudes towards the significance of the independence results.

Ernst Zermelo's paper of 1908 entitled "Investigations in the foundations of set theory I" [1908a] is generally credited as presenting the first axiomatization of set theory. His axioms, in one form or another, may still be said to constitute the core of the axiomatic approach to set theory. In Part II, after highlighting the distinction between what I refer to as the logical and mathematical approaches to set theory, I discuss some of the central features of the transformation of Zermelo's axioms into the formal system ZF. Specifically Skolem's contribution to this transformation.

The transformation of Zermelo's axioms into ZF comprises a sequence of systems featuring addition of axioms e.g. axiom of foundation, differences in the formulation of particular axioms, differences in the degree of formalization, changes in the background logic as well as the underlying heuristic, e.g. influence of the 'iterative' notion of set. Amongst other things, an examination of this transformation facilitates a discussion of what is purportedly being formalized by ZF.

Now ZF and the axiomatizations and formalizations of set theory which are its precursors *are* putative axiomatizations and formalizations of an informal body of mathematics. In a rather broad sense this informal body of mathematics may be identified as the Cantorian notion of set. But underlying these progressive formalizations, although *generally* centred on the Cantorian notion, we are not faced with a fixed concept of set but rather a notion in the process of evolution. It is clear from a critical account of developments from Cantor to Cohen (and certain post-Cohen developments e.g. elementary topos theory) that the notion has undergone significant transmutations. Moreover, the aforementioned evolution has to a large extent been driven by a feedback mechanism in the sense that the *process of formalization itself has served to modify the informal notion.*

The crucial point in the progress of the formalization of set theory occurs in the transformation of Zermelo's system following Skolem's revolutionary paper of 1922. The main body of Skolem's paper consists of remarks upon eight "points". Skolem lists these as:

1. The peculiar fact that, in order to treat of "sets", we must begin with "domains" that are constituted in a certain way;

2. A definition, much to be desired, that makes Zermelo's notion "definite proposition" precise;

3. The fact that in every thoroughgoing axiomatization set-theoretic notions are unavoidably relative;

4. The fact that Zermelo's system of axioms is not sufficient to provide a foundation for ordinary set theory;

5. The difficulties caused by the nonpredicative stipulations when one wants to prove the consistency of the axioms;

6. The nonuniqueness of the domain *B*;

7. The fact that mathematical induction is necessary for the logical investigation of abstractly given systems of axioms;

8. A remark on the principle of choice. [1922 pp.291-292]

In this paper Skolem proposed the key changes that marked the transformation of Zermelo's system into ZF, the protagonist in Cohen's theorems. Moreover, Skolem's criticisms in this paper raise fundamental philosophical problems for set theory, specifically problems with respect to its axiomatization and formalization. Most contemporary papers on the philosophical problems of set theory relating to its formalization deal with issues that can be directly traced back to those raised in Skolem's 1922. For example, taking Skolem's first remark as a starting point, is Mayberry's 1977 whose criticisms and proposals for an alternative axiomatization of set theory are underpinned by his interpretation of Cantorianism, in particular Cantor's 'finitism'.

In addition, although the flood of independence results initiated by Cohen in 1963 brought certain foundational issues into sharp relief, it did not create essentially new foundational problems but rather it dramatically underlined problems that were inherent in the enterprise of formalizing set theory from the beginning. Moreover, they brought to centre stage issues which had for the most part been ignored due largely to the successful and important contribution of set theory to the development of twentieth century mathematics. That many of these issues are not new is clearly evidenced by Skolem's paper. For this reason it offers not only a springboard for a discussion of current views upon the issues raised by his remarks but also an excellent focus for an examination of the genesis of ZF. However, in order to appreciate the points that Skolem was making, it is also necessary to discuss relevant aspects of Zermelo's 1908.

It is the topic of 'definite properties' that receives centre stage in part II. This is for the following reasons:

i) the treatment of definite properties *is a central feature* of the formalization of set theory. At the same time, however, it does not receive sufficient attention in the literature. For example, there has recently appeared two generally excellent (and large) books on the history and philosophy of set theory, namely: Moore 1982 and Hallett 1984, both of which discuss the formalization of set theory but have failed to recognize, or at least make evident, the full importance of the notion of 'definite property' in this connection.

ii) quite a few of the issues raised in Skolem's other remarks are essentially tied to this topic.

iii) the treatment of 'definite properties' is intimately related to independence results in set theory. As Bernays puts it

What the applicability [of Cohen's results] ...depend[s] on is the...sharper axiomatization by which strict formalization becomes possible, that is, the Fraenkel-Skolem delimitation of Zermelo's concept '*definite Eigenschaft*' [1967 p.117]

THE MATHEMATICAL AND LOGICAL APPROACHES TO SET THEORY.

It is important to distinguish two approaches to the theory of sets, the logical and mathematical, and recognize that Zermelo's system and the various systems stemming from it, through to ZF, belong to the latter. *The essential characteristic of the mathematical approach is its concern with the Cantorian theory of transfinite arithmetic.* Cantor, as Hallett informs us,

> was the founder of the mathematical theory of the infinite, and so one might with justice call him the founder of modern mathematics. Certainly a large part of his achievement was to help make the notion of set the basic one in mathematics. But in many ways the core of his work was his theory of transfinite number, especially the concept of *ordinal* number....to understand *why* the ordinals were so important we have to go back to some of Cantor's earlier work and the problem of powers or infinite sizes that it raised.
>
> The problem of powers in its most general form is to find a calculus of absolute size (power) adequate for describing the sizes of arbitrary infinite sets. [1984 p.1]

Cantor's theory of transfinite arithmetic was his solution to the "problem of powers". This theory may be characterized, albeit rather simplistically, as the extension of the natural numbers and the arithmetical operations upon them, i.e. addition, multiplication and exponentiation into the transfinite. The constituents of this extension were the transfinite ordinals. The cardinals were defined in terms of the ordinals. Cantor considered the fundamental application of his theory was to be the determination of the power of the continuum.

As I stated above, Zermelo's system and its various transformations through to ZF is Cantorian. Certainly, Zermelo, Skolem, Fraenkel and von Neumann professed their systems so to be. Of course, the extent to which these various systems, in particular ZF, *are* Cantorian is problematic and the sense of their Cantorianism requires analysis. That is, Cantorianism incorporates a transfinite arithmetic, a philosophy of the infinite and general heuristic principles, e.g. the priority of the ordinal numbers; and each system needs to be examined with respect to its incorporation of these particulars. But it is clear that they are to be included within the mathematical approach.

Zermelo, for instance, states in the introduction to his 1908a that in this paper his intention is

to show how the entire theory created by Cantor and Dedekind can be reduced to a few definitions and seven principles, or axioms,...[p.200]

Zermelo, in claiming to have reduced Cantor's "entire theory", is referring to Cantor's transfinite arithmetic. In fact, the greater part of Zermelo's paper is taken up with developing the 'theory of equivalence' i.e. fundamental cardinal theory. Thus, having presented his system and showing that Russell's antinomy is blocked, Zermelo's immediate concern is with establishing the adequacy of his system for the reconstruction of transfinite arithmetic.

Transfinite arithmetic does *not* feature in the *essential* concerns of the logical approach to set theory. Investigating the general theory of domains which underpins the semantics of logic, e.g. the legitimate ranges of quantifiers, or a general theory of the extensions of properties are paradigms of the logical approach. This approach is to be found in the work of Boole, Bolzano, and the logicists Frege and Russell. Logicists, not surprisingly, tend to construct unified integrated systems of logic and sets.

Underpinned by a version of the vicious circle principle, Russell's theory of types, in contrast to Zermelo's system, was a genuine and direct response to the *logical* problem of the incoherence of the unrestricted axiom of comprehension. More generally, after the antinomies, the logical approach features a concern with a conceptual analysis of the set concept obviating the antinomies and in Russell and Poincaré's case the requirement that a unified non *ad hoc* explanation be given of the known antinomies. That is, an analysis yielding a principle whose violation is the root cause of both the logical and semantic paradoxes. Poincaré's investigations, which also led him to formulate a form of the vicious circle principle, were based on the analysis of a semantic paradox. (In anticipation, I note that consideration of the semantic paradoxes played a more prominent role in the formative stages of set theory, particularly within the mathematical approach, than is generally acknowledged.)

Both the approaches incorporate the thesis that set theory is sufficient as a foundation for mathematics. For the logicists this was a matter of necessity. Those within the mathematical approach tended towards an instrumentalistic attitude and, in general, were not inclined towards intensive philosophical analysis. Significantly, this instrumentalistic attitude emerges in connection with the formulation of the notion of definite properties which was basically a matter of *stipulation* without, for instance, the benefit of any accompanying attempt at a philosophical analysis of 'property'.

By way of illustration of foundational attitudes consider the following two quotations. First, the opening passage of Zermelo's 1908a:

> Set theory is that branch of mathematics whose task is to investigate mathematically the fundamental notions "number", order", and "function", taking them in their pristine, simple form, and to develop thereby the logical foundations of all of arithmetic and analysis; thus it constitutes an indispensable component of the science of mathematics. [p.200]

Second, from Mayberry's 1977

> The function of set theory in the foundations of mathematics is a logical one. It is essentially a theory of definitions and arguments. It provides the raw materials (i.e. sets, functions, ordered pairs, *etc.*) and formal techniques for the definition of mathematical structures and, through the unpacking of these definitions, the ultimate principles upon which mathematical argument rests. [p.18]

As indicated in the latter quotation, the reducibility of mathematical notions to those of set theory constitutes a key component of the foundational thesis. For the logicists a reductionist programme was a necessary undertaking. With the possible exception of full category theory, it is generally acknowledged that the reductionist programme has been realized. The credit for this extraordinary success is chiefly due to the logicists. (One notable exception is the work of Hausdorff and Kuratowski on reducing 'function', 'relation' and 'ordered pair'.) The reductionist programme, taken on board (to various degrees) within the mathematical approach in a largely instrumentalistic spirit, may thus be said to be parasitic on the work of the logicists. More generally, if the metatheory within which we are considering some formal system constructed within the mathematical approach is set theoretic then there is no bar to the metatheory being based on the work of the logical approach. It is possible that this metatheory could turn out to be relatively weak. There may, for instance, be no necessity to incorporate a transfinite arithmetic into the metatheory. Or there may be large cardinals in the object theory and not even a replacement principle in the metatheory. Indeed, it is arguable that nesting the metatheory within the logical approach would be philosophically sound.

The logical approach, particularly that of the logicists, is close in spirit to the Leibnizean tradition and to some extent arises directly out of the Leibnizean programme of developing a *characteristica universalis*. As Dumitriu observes

> ...the art of the characteristic was essentially connected to the art of demonstrating, to vocabulary, and thus to the language itself, as Couturat underlined. Liebniz had to establish a universal language, or vocabulary, in order to be able to start the algebraical mechanism of the characteristic. Here is what Couturat stated about this matter (*La Logique de Leibniz*, p.79): "In order to set up the alphabet of human ideas, which had to be the vocabulary basis, all concepts had to be analysed and reduced to simple elements using definitions. But that meant taking stock of all human knowledge, and, as the analysis of concepts is the analysis of truth, it also meant demonstrating all known truths, reducing them to simple and evident principles...[1977 Vol. III p.141. See also Couturat 1901]

Constrained to mathematics Leibniz's programme is the blueprint for Frege's *Begriffsschrift*. In fact in this connection Frege wrote

My intention was not to represent an abstract logic in formulas, but to express a content through written signs in a more precise and clear way than it is possible to do through words. In fact, what I wanted to create was not a mere *calculus ratiocinator* but a *lingua characterica* in Leibniz's sense. [1882 pp.1-2. Quoted in van Heijenoort 1967 p.2]

Frege in the passage above is differentiating his approach from Boole's. Boole tended to concentrate on the algebraic structure of disciplines such as logic, arithmetic, probabilities and sets and then highlighting their structural similarities. Moreover, in applying any perceived structural similarities, he emphasized the method whereby one works with a particular algebra (usually arithmetical or perhaps uninterpreted in the sense that he worked formally with the operations) then after making the required calculations there is a choice of interpretations of the result. A typical example occurs in his 'Laws of Thought' [1854] where on determining the structural identity between his algebras of logic and the arithmetical algebra of 0 and 1 Boole stresses that

We may in fact lay aside the logical interpretation of the symbols in the given equation; convert them into quantitative symbols, susceptible only of the values 0 and 1; perform upon them as such all the requisite processes of solution; and finally restore to them their logical interpretation. [p.70]

The contrast between Frege and Boole is an early indicator of the emergence of an important facet of the mathematical approach: namely an algebraic treatment of mathematical concepts. [Its significance is taken up in part III chapter 3.]

Both approaches were party to the general trend towards formalization in the latter half of the nineteenth century. For the logical approach this was part of the Leibnizean influence. As Wedburg observes

When Leibniz dreamed about his *characteristica universalis*, and when he sketched his many fragmentary logical calculuses, he seems to have had in mind the notion of an effective and perhaps also decidable calculus. [1984 p.276]

The greatest overall influence on the mathematical approach with respect to formal developments derived from Hilbert. Cantor does not figure here. Following the founding of modern logic by Boole, De Morgan and Bolzano the theory was developed throughout the century. Peirce , Schröder, Peano and Frege being amongst the principal contributors. Much of this work was available to Cantor. However, as Moore informs us

Little concerned with axiomatic systems or with logic in general, Cantor did not rely on Boole's investigations or those of Boole's successors. Nor did he conceive of his results within a formal system, such as the one that Frege proposed in his *Begriffsschrift*...
[1980 p.101]

So in this respect the mathematical approach was not at all Cantorian.

On the other hand the mathematical approach incorporates Cantor's instrumentalism. I mentioned above that the reductionism, common to both approaches, was, within the mathematical approach, undertaken in an instrumentalistic spirit. The set theories of Cantor and Dedekind, which Zermelo cites as underlying his system, were also developed with an instrumentalistic attitude in the following sense. The set theories of Dedekind and Cantor both derived from their work on *specific* *mathematical problems.* In Cantor's case this was the problem of the uniqueness of the representation of an arbitrary function by a trigonometric series. Dedekind's set theory originated from his desire to give a proof of the Bolzano-Weierstrass theorem that had no recourse to geometric intuition. This was part of his programme to find, as he put it,

a purely arithmetic and perfectly rigorous foundation for the principles of infinitesimal analysis. [1872. Translation by Beman 1909 p.2]

The intermingling of strands within the two approaches derives from an overlapping concern with foundational questions. It must be emphasized, however, that the drive of these foundational concerns came from different directions. Cantor and Dedekind were undertaking a conceptual analysis from within. That is, their overall concern was motivated by *internal* problems of mathematics. On the other hand, Frege and Russell for instance, although both had mathematical backgrounds, were essentially philosophically motivated. Their aim was to prove the logicist thesis.

Some tendency for certain strands to interweave is made manifest when we note that some commentators place Dedekind within the logicist camp. Gillies, for instance, in comparing Dedekind with Frege states

The main point of similarity with Frege is that Dedekind also espouses logicism. [1982 p. 50]

Indeed, there are passages in Dedekind's works underwriting such a judgement. For example, in his paper 'Was sind und was sollen die Zahlen?' he writes

In speaking of arithmetic (algebra, analysis) as part of logic I mean to imply that I consider the number-concept entirely independent of the notions or intuitions of space and time, that I consider it an immediate result from the laws of thought. My answer to the problem propounded in the title of this paper is, then, briefly this: numbers are free creations of the human mind; they serve as a means of apprehending more easily and more sharply the difference of things. It is only through the purely logical process of building up the science of numbers and by thus acquiring the continuous number-domain that we are prepared accurately to investigate our notions of space and time by bringing them into relation with this number domain created in our mind. [1888 pp. 31-32]

But following through, Gillies comments on this passage as follows

Dedekind like Frege rejects the Kantian theory of arithmetic. However we note at once one difference from Frege. Dedekind has a psychologistic rather than Platonistic view of logic. He speaks of "the laws of thought" and of numbers as being "free creations of the human mind." Another difference between the two emerges later. Dedekind regards the notion of class, or set, or to use his own terminology, system, as a logical notion. But Frege denies this. In his preface to [1893] *Grundgesetze der Arithmetic* Vol. 1, Preface Furth translation p. 4 Frege writes:

"Herr Dedekind, like myself, is of the opinion that the theory of numbers is a part of logic; but his work hardly contributes to its confirmation, because the expressions "systems" and "a thing belongs to a thing", which he uses, are not usual in logic and are not reduced to acknowledged logical notions."

For Frege, "concept" and "extension of a concept" are logical notions, whereas "set", "class", "system" are not. Thus Frege's point of view leads to higher order logic and type theory; whereas Dedekind's leads to axiomatic set theory. [(1982); p. 51)]

I suggest that an analysis of the interplay of the foundational consequences of Frege's and Dedekind's points of view, as presented above, is a prerequisite to an appreciation of the philosophical and foundational problems of axiomatic set theory. But equally, if not more, important is a recognition of the distinction between these views in so far as it is manifested in the mathematical and logical approaches.

Now Zermelo was *not* the first to axiomatize set theory. The systems of Dedekind, Frege and Russell, for instance, were not preceeded by Zermelo's. However, Zermelo's system *is* the axiomatic starting point of the mathematical approach. That it is generally held to be the original axiomatization of set theory emphasizes the *apparent* predominance of this approach. This apparent predominance is certainly the case so far as the development of that set theory converging to ZF is concerned. Undoubtedly ZF (the protagonist of Cohen's results) is a crucial stage in the evolution of set theory within the mathematical approach. In discussing general foundational issues concerning ZF, and in particular the foundational significance of Cohen's results it is appropriate and important that ZF be viewed in this context. Finally, in anticipation, my proposal for post-Cohen set theory in Part III is posited as a natural continuation of this evolution within the *mathematical* approach. I now turn to its axiomatic origins.

ZERMELO AND AXIOMATIZATION.

In his introductory note to Zermelo's 1908a van Heijenoort makes some apparently straightforward and uncontroversial observations on Zermelo's axiomatization. However, it is worthwhile reconsidering and, in some instances, questioning them. Van Heijenoort states that

> [Zermelo's] paper presents the first axiomatization of set theory. Cantor's definition of set had hardly more to do with the development of set theory than Euclid's definition of point with that of geometry. Dedekind, whom Zermelo considers one of the two creators of set theory had explicitly stated...a number of principles about sets (which he called "systems"), but his attempt had remained fragmentary and had been somewhat discredited by the nonmathematical way in which he justified the existence of an infinite set...In spite of the great advances that set theory was making, the very notion of set remained vague. The situation became critical after the appearance of the Burali-Forti paradox and intolerable after that of the Russell paradox, the latter involving the bare notions of set and element. One response to the challenge was Russell's theory of types...Another, coming at almost the same time, was Zermelo's axiomatization of set theory. The two responses are extremely different; the former is a far reaching theory of great significance for logic and even ontology, while the latter is an immediate answer to the pressing needs of the working mathematician. [1967 p.199]

The main point I comment on here is the generally accepted view that Zermelo presented an *axiomatization* of set theory in response to the appearance of the set theoretical paradoxes and in particular to those mentioned in the above passage. But I begin with some remarks on some of the other observations of van Heijenoort's.

Van Heijenoort points out that the responses of Russell and Zermelo to the paradoxes are "extremely different". The difference he indicates in this case is consonant with the general distinction I have made between the logical and mathematical approaches including the instrumentalistic tendency of the latter.

In line with the popular view, van Heijenoort informs us that Zermelo's is the first axiomatization of set theory. Now the sense in which it is correct to assert that Zermelo's system was the first axiomatization of set theory and its significance was taken up in the discussion on the two approaches to set theory. But we may further pursue its import if we consider it in conjunction with van Heijenoort's following assertion i.e. "Cantor's definition of set had hardly more to do with the development of set theory than Euclid's definition of point with that of geometry" and that both Cantor and Dedekind were considered by Zermelo to be the creators of set theory. To begin with both Cantor and Dedekind provided 'definitions' of 'set'. It is of interest to make a comparison. Dedekind writes

> It very frequently happens that different things, ...for some reason can be considered from a common point of view, can be associated in the mind, and we say that they form a *system*. [1888 translated by Beman 1901 p.45]

Cantor provided several versions. The one from his 1895 reads

> By a 'set' we understand every collection to a whole *M* of definite, well-differentiated objects *m* of our intuition or our thought. [Translation in Hallett 1984 p.33]

Now what underlies van Heijenoort's assertion? Put bluntly, it is that Zermelo's system is posited as an implicit definition of set. [This point is taken up in Part III, chapter 3] Here Zermelo is displaying the Hilbertian attitude to axiom systems evident in the '*Grundlagen* *der Geometrie*' [1899]. Now Euclid's system comprises axioms, postulates and definitions and as Gorsky puts it

In Euclid's *Elements* the separate 'primitive' terms of the system are given explicit definitions. These definitions lie beyond the system itself...The terms in which the definitions of a number of primitive concepts are described do not appear in the formulations of the relevant primitive propositions (postulates and axioms) or in the proofs of the theorems, therefore such terms in Euclidean geometry as 'point', 'straight line', 'surface', 'plane' should be regarded as primitive terms introduced without definitions. In proving the relevant theorems only those properties of the terms are taken into account which are described by the corresponding primitive propositions...[1974 p.43]

Given Hilbert's view of the status of axioms a definition of 'point', for example, added to his axioms for Geometry would make no sense. Van Heijenoort is tacitly incorporating the Hilbertian view together with that suggesting that mathematical disciplines are developed by means of such axiom systems. So Cantor's definition of set plays no more role in the development of set theory (from Zermelo's system) than does a definition of 'point' in the development of Euclidean geometry since (Zermelo's) axioms constitute an implicit definition.

(i) Zermelo and the Logical Paradoxes.

I now turn to the question of Zermelo's purpose in formulating an *axiomatic* theory of sets. Why was he motivated to formulate an axiomatic system? According to van Heijenoort's account Zermelo's axiomatization was a direct response to the "challenge" of the paradoxes of Burali-Forti and Russell. Indeed, this is no more than Zermelo professes in the introduction to his 1908a. He explains that

> At present... the very existence of this discipline [set theory] seems to be threatened by certain contradictions, or "antinomies", that can be derived from its principles - principles necessarily governing our thinking, it seems - and to which no entirely satisfactory solution has yet been found. In particular, in view of the "Russell antinomy" ...of the set of all sets that do not contain themselves as elements, it no longer seems admissible today to assign to an arbitrary logically definable notion a set, or class, as its extension. Cantor's original definition of a set (1895) as "a collection, gathered into a whole, of certain well-distinguished objects of our perception or our thought" therefore certainly requires some restriction; it has not, however, been successfully replaced by one that is just as simple and does not give rise to such reservations. Under these circumstances there is at this point nothing left for us to do but to proceed in the opposite direction and, starting from set theory as it is historically given, to seek out the principles required for establishing the foundations of this mathematical discipline. [p.200]

Zermelo's own account of the matter has been accepted and reiterated more or less universally by commentators. As Moore aptly puts it: "It has become part of the mathematical and philosophical folklore". But is Zermelo's account misleading? Does the standard account stand in need of revision? I maintain that the answer to these questions is in the affirmative. For Zermelo is ostensibly axiomatizing the Cantorian notion of set. But the Cantorian notion of set is *not* inconsistent. Or at least its inconsistency cannot be demonstrated by means of those principles invoked in the construction of the Burali-Forti or Russell paradox. However, the belief that Cantor's notion of set does gives rise to these paradoxes has been widely held. Furthermore, it continues to be passed on by generally astute contemporary commentators. For example, we find the following passage in Kitcher's 1983

> On several occasions in the past, mathematicians have hailed some principles as intuitively evident, giving them the status that we give to the axioms of set theory. It has then turned out that these principles are false. The most familiar example is that of Frege, Dedekind, and Cantor, each of whom advanced a universal comprehension principle, taking any property to determine a set. [p.63.]

It is thus important to clarify the status of the Cantorian notion in this connection. Hallett does so in his 1984. There he observes that

> It is sometimes assumed that Cantor's 'definitions'... allow virtually any collection to be a set, and therefore that Cantor's system clearly gives rise to the famous paradoxes, say those of Burali-Forti or Russell...that given the framework of a first order logical calculus, Cantor's 'definition' translates into the so-called 'comprehension principle', that is the axiom schema according to which each predicate yields an axiom $\Sigma x \Pi y[y \in x \leftrightarrow \phi(y)]$. But this view is quite mistaken, Cantor's 'definitions' only allow as sets those collections which are *whole* and this does not at all imply that any collection can be a set. Nothing like the comprehension principle of so-called 'naive set theory' follows from Cantor's statements. If 'naive set theory' is characterized as set theory based on the comprehension principle, then this goes back, not to Cantor, but to Russell (1903) [p.38 See also Mayberry 1977,1980; Moore 1978, 1982; Dauben 1979 and even Hilbert 1904.]

As far as set theoretic antinomies are concerned, Cantor was well aware of the problem they might present. He did not, however, consider that his theory of sets was thus rendered not viable. In his philosophy of the infinite Cantor distinguished between two types of infinite collection. Namely, the *transfinite* and the *absolutely infinite*. Transfinite collections were 'extendable' or 'bounded'. They were mathematically viable in that they could be considered as having a 'number' associated with them, i.e. a cardinality - this being analogous to the case of finite collections. Absolutely infinite collections were *not* extendable and it was the ascription of cardinality to such a collection, i.e. considering it to be transfinite, which led to antinomies. Thus, Cantor sometimes made the distinction between the transfinite and absolutely infinite in terms of 'consistent' and 'inconsistent' collections. Furthermore

> Instead of regarding these absolutely infinite or inconsistent collections as a source of paradoxes, he treated them as tools with which to obtain new mathematical results. [Moore 1980 p.104 N.B. This method was adopted by Zermelo and has in fact become a standard technique in the development of set theory particularly after the incorporation of von Neumann's ordinals and definitions by transfinite induction. For a relatively recent example of its use see Scott 1974]

An important example of this usage was Cantor's demonstration, to be found in his 1899 letter to Dedekind, of the well-ordering of the ordinals. Significantly, as Moore emphasizes

> What is intriguing about this demonstration, in terms of the relationship between logic and set theory, is Cantor's recognition that his concept of set cannot be identified with the most general concept of class or collection. [1980 p.103]

So if Cantor's notion of set did not allow the Burali-Forti and Russell paradoxes and if Zermelo was axiomatizing the Cantorian notion it cannot be that the standard view of Zermelo's motivation is credible. An alternative view has recently been put forward.

(ii) Moore's Argument.

In his 1978, 1980, and 1982 Moore proposes and develops the argument that

> Zermelo was primarily motivated to axiomatize set theory not by the paradoxes but by the controversy surrounding his proof that every set can be well-ordered and especially by a desire to secure his axiom of choice against its numerous critics. [Moore 1980 p.106]

To appreciate this argument it is necessary to look at the important place of the well-ordering principle within the mathematical approach to set theory and Zermelo's involvement with its defence.

Cantor's theory facilitates the characterization of a set with an ordering relation on it as being well-ordered if and only if it is order-isomorphic to an ordinal. Now we may take the well-ordering principle as stating that for every set X there exists an ordering relation R on it and an ordinal α such that X is order-isomorphic to α relative to R. But why was this principle of such importance to Cantor?

We mentioned above that Cantor defined the cardinals in terms of the ordinals. The cardinals were seen as *the fundamental* measure on all mathematical objects. That is, the role of the cardinals was to act as a scale of the powers of transfinite sets just as the natural numbers act as a scale of the sizes of the finite sets. In order for the scale to be viable the following three conditions had to be met:

(i) Completeness, i.e. given a transfinite set its power must be a 'point' on this scale.

(ii) Linear ordering. (In fact the scale was well-ordered.)

(iii) Uniqueness, i.e. the power of a transfinite set is associated with at most one cardinal.

The construction of a system of arithmetic for the ordinals and cardinals is the essential task within the mathematical approach and a scale for powers is a key step towards this end. Given the manner in which Cantor's theory produced cardinals from ordinals the well-ordering principle was the crucial premiss in the demonstration that the aforementioned scale was indeed viable. In fact, we could go further in that it is arguable that the well-ordering principle lies at the heart of the classical mathematical approach to the theory of sets.

Clearly anyone concerned with Cantor's theory (and we have claimed Zermelo to be such a one) would be interested in the foundational status of the well-ordering principle. Cantor originally viewed it as a principle of logic, i.e. as a law of thought. About 1895, however, he appeared to have changed his mind about this and at the same recognized the need for, and indeed attempted, its demonstration. So too did Zermelo.

Zermelo's active involvement with the question of the well-ordering principle appears to have been initiated in 1904 at the Third International Congress of Mathematicians. The catalyst was Zermelo's confutation of a demonstration offered by Julius König that the continuum could *not* be well-ordered. "That 'proof', expressed in the technical language of Cantorian set theory, stunned the Congress and especially Cantor." [Moore 1978 p.310]. But by the next day Zermelo found that a crucial step in König's argument was invalid. That is, it was an application of a result from Bernstein's recently completed thesis, namely: $(\aleph_\alpha)^{\aleph_0} = (\aleph_\alpha).2^{\aleph_0}$, which was in fact not proved in the case where α is a limit ordinal. Unfortunately for König, this was the very case he relied on in his demonstration. Following this incident Zermelo worked on the well-ordering theorem and within two months had formulated a proof which was published later that year. This proof made essential use of Zermelo's 'Axiom of Choice' and its occurence in the published proof is the first explicit formulation of this axiom. [See Zermelo 1904]

Subsequent to the publication of Zermelo's proof there ensued an intense international debate over its details. Some of the most prominent mathematicians of the time participated in this debate and Zermelo certainly was very much taken up in considering the diverse criticisms levelled against his proof. That these criticisms were quite diverse, as we shall see below, needs to be stressed. Briefly, they included the criticisms of the 'semi-intuitionists' Baire, Borel, and Lebesgue along (loosely speaking) constructivist lines; Poincaré's objection that Zermelo had employed an impredicative definition; Peano's objection on logical grounds that the axiom of choice is not valid in the infinite case if the choices are arbitrary; and the set-theoretical arguments of the German Cantorians Bernstein and Schoenflies.

Moore summarizes his argument as follows:

> If the paradoxes were not the main factor motivating Zermelo to invent his axiomatization, then what was that factor? As we have seen, between 1904 and 1907 Zermelo's Axiom of Choice and his 1904 proof had been subjected to numerous extended criticisms by many eminent mathematicians. Throughout his career, controversy spurred Zermelo to his greatest efforts, and this was particularly the case for his axiomatization. Of course, he wanted to place set theory on a firm axiomatic foundation, which would in turn serve as the basis for all mathematics. But the evidence shows that by 1904 he regarded the paradoxes as only an apparent threat.
>
> What *was* threatened, was the acceptance of his proof that every set can be well-ordered. How could that proof be secured? Zermelo's answer was two-fold. First, he replied to his critics at length and gave a new proof, which nevertheless depended as heavily as the first on his Axiom of Choice. Second, he created an axiomatization of set theory and embedded his proof within it. In order to preserve his entire proof, his axiomatization needed to include his Axiom of Choice, as he realized very well. Indeed that Axiom - the first part of his axiomatization to be formulated explicitly - intrigued him because of its fruitfulness, even outside of his proof. [1978 p.327]

Moore has presented a forceful and richly illustrated argument and has certainly been convincing in so far as obviating the above-mentioned folklore. Moreover, I submit that the propositions, *viz*:

(*) the well-ordering principle was of central interest to the development of Cantorian set theory;

(*) Zermelo did not perceive the known paradoxes as a serious threat to the Cantorian notion of set and particularly to the construction of Cantor's transfinite arithmetic;

(*) Zermelo, after the publication of his 1904, was greatly concerned with countering its numerous critics and produced his 1908 in response;

which form the foundation of Moore's argument, are correct. In fact, only the second of these has been controversial. But Moore's contention that Zermelo's reply was "two-fold" is problematic. I suggest that although Moore is correct in highlighting that Zermelo's 1908 and 1908a were "interrelated in numerous ways" he is incorrect to conclude that in any strong sense they were "in conception and motivation... a single paper." In other words I shall argue that Zermelo's 1908a, although in certain respects an extension of his 1908, does *not* constitute part of Zermelo's riposte to the critics of his 1904 proof and axiom of choice.

Zermelo's 1908 comprises two parts. The first a new proof of the well-ordering theorem the second his reply to the objections against his first proof. Thus we could begin by arguing that Zermelo's 1908a in so far as it is a counter-argument to his critics is at best a supplement to an already *sufficient* response. Sufficient, that is, so far as Zermelo is concerned. Moreover, in that it does not offer any qualitatively different counter-arguments to his critics this sheds doubt on the contention that Zermelo's motivation in formulating it was to answer his critics. The tack I shall pursue runs as follows: if we look at the *nature* of the criticisms taken up by Zermelo we shall see that it is simply not convincing that he constructed his axiomatization to provide a counter-argument to them.

I have noted that the criticisms were diverse. However, Zermelo informs us in his summary that

The preceding discussion of the opposition to my 1904 proof can perhaps be summarized most simply by the following statements. Except for Poincaré, whose critique, based on formal logic - a critique that would threaten the existence of all of mathematics - has hitherto not met with any assent whatsoever, all opponents can be divided into two classes. Those who have no objection at all to my deductions protest the use of an unprovable general principle, without reflecting that such axioms constitute the basis of every mathematical theory and that precisely the one I adduced is indispensable for the extension of the science in other respects, too. The other critics, however, who have been able to convince themselves of this indispensability by a deeper involvement with set theory, base their objections upon the Burali-Forti antinomy, which in fact *is without significance* for my point of view, since the principles I employed *exclude* the existence of a set Ω. [1908 p.198. N.B. Ω is the collection of all ordinals]

I first consider those criticisms which attack Zermelo's proof by invoking the spectre of the Burali-Forti paradox. Here the worry is not that his proof is lacking in rigour but that it is trivial in the sense that it's premisses are contradictory. Zermelo's answer is that these objections are misconceived because they presume he treats Ω as a set. He states

I clearly restricted myself to principles and devices that have not yet by themselves given rise to any antinomy. If some critics nevertheless deploy this ominous "set Ω" against my proof, they must first project it into that proof artificially, and all arguments drawn from the inconsistent character of this "set" turn back upon their authors. [1904 p.192]

Zermelo's axioms incorporate the premisses of his proof. Now suppose Zermelo's premisses *are suspected of being* contradictory on the grounds that they allow the Burali-Forti argument. Then how does embedding them in his axioms facilitate a counter-argument? One way would be to provide a consistency proof of the axioms. But Zermelo admits "I have not yet even been able to prove rigorously that my axioms are consistent". [1908a p.200] Another alternative is to demonstrate that at least the axioms do not allow the reproduction of the Burali-Forti argument. But Zermelo offers no such demonstration. In fact, as Moore points out that in his 1908a "Zermelo devoted minimal space to discussing the paradoxes". [1978 p.325] This seems to indicate that countering this particular attack on his proof was not the motivation behind Zermelo's axiomatization. Incidentally, if it was then in this sense it contradicts Moore's contention that the paradoxes were not Zermelo's primary motivation.

Axiomatization was certainly not a counter-argument to critics of his axiom of choice. Even in his 1904 Zermelo concedes that "This logical principle cannot, to be sure, be reduced to a still simpler one..." [p.141] In his 1908 he emphatically repeats that "I just cannot *prove* this postulate...and therefore cannot compel anyone to accept it apodictically." [p.186] His defence of his axiom of choice is an appeal to its self-evidence its necessity for many "elementary and fundamental theorems and problems" of extant mathematics. Furthermore, he states that

...in order to reject such a fundamental principle, one would have had to ascertain that in some particular case it did not hold or to derive contradictory consequences from it; but none of my opponents has made any attempt to do this. [1908 p.187]

Thus it seems that there is no case for an axiomatization to answer. Nevertheless, let us suppose that Zermelo axiomatized to secure his axiom, not against criticisms that had been in fact been offered, as strictly speaking, Moore suggests, but to forstall any of the kind mentioned in the above passage. More specifically, that it did not lead to contradictions when in the context of Cantorian set theory. For this to go through he would need a consistency proof. But he didn't have one! It is possible, however, that he assumed one would be forthcoming without too much delay. (Note also that Zermelo, in precocious mood, asserts "this postulate is logically independent of the others" [p.187] and so he is ready to countenance counterexamples but one assumes he would understand them to be pathological relative to 'classical' mathematics. In any case, an axiomatization of set incorporating the axiom is not an *argument* against counterexamples.)

We are now left with Poincaré's criticism that Zermelo's proof is unacceptable since it employs impredicative definitions. How does an axiomatization answer this charge? Well, it might provide the means for formulating a proof not employing such definitions. But this is extremely doubtful for several reasons. To begin with, there is no hint or allusion offered by Zermelo of such a proof. Another reason is Zermelo's general instrumentalistic attitude. He took Poincaré's view to be untenable since if acted upon it would emasculate classical mathematics. In fact, he thought "the strict observance of Poincaré's demand would make every definition, hence all science, impossible." [1908, p.191] Thus Zermelo is not motivated to provide a framework for a proof in accord with Poincaré's strictures.

In the light of the above considerations Moore's answer to the question "what gave rise to Ernst Zermelo's axiomatization of set theory in 1908?" is unconvincing. Coupled with the fact that Zermelo had comprehensively answered his critics in his 1908 the question of why Zermelo went on to write the following paper still remains unsettled. I conclude this chapter with some remarks hopefully resolving the issue.

(iii) Resolution: Hilbert and axiomatization.

I have argued that Zermelo was not primarily motivated to axiomatize set theory because of the paradoxes nor to defend his proof of the well-ordering principle and his axiom of choice. Whilst there is probably an element of truth in both these accounts I contend that the fundamental impulse to axiomatize derived straightforwardly from the desire, *for general foundational considerations*, to bring Cantorian set theory within the fold of those theories to which the axiomatic method had been applied. In other words, to codify and precisify Cantorian set theory. Prosaic as this might seem, this was indeed the case.

Zermelo was working under Hilbert's influence, and Zermelo's axiomatization was a continuation of the work initiated by Hilbert after his axiomatization of geometry in his 1899. That is, to treat the central disciplines of mathematics, e.g. real number theory, as he had Euclidean geometry. Moore is not unaware of Zermelo's Hilbertian leanings during this period in his career. In fact, he informs us that

> Coming from Berlin, where he had worked in mathematical physics, Zermelo obtained his *Habilitationsschrift* in the same field at Göttingen during 1899. Under Hilbert's influence, which Zermelo later described as the most important of his mathematical career, his interests soon turned to set theory and the foundations of mathematics. [1980 p.105]

We also find in his 1978

> Like Hausdorff and unlike Russell, Zermelo considered set theory as a part of mathematics, rather than philosophy or logic. But unlike

Hausdorff, Zermelo regarded an axiomatization of set theory as essential to its sound future development. [p.324]

and

Of course, he [Zermelo] wanted to place set theory on a firm axiomatic foundation, which would in turn serve as the basis for all of mathematics. [p.326]

Moore is not explicit as to why the locutions "of course" and "essential" are warranted here; and, after all, they are not warranted so far as Hausdorff is concerned. But now from these quotations there clearly emerges the driving force behind Zermelo's axiomatization. Like Hilbert, he considered axiomatization, *per se*, to be a key foundational exercise. So given the general importance of Cantorian set theory this is powerful enough motivation for Zermelo.

To conclude this section and as a bridge into the next chapter I make some further remarks arising from the above discussion.

(i) I mentioned above that in certain respects the 1908a paper is an extension of the 1908. Generally speaking, the earlier paper provides an aid for interpreting the later one. More specifically, the principles listed at the beginning of the 1908, in a slightly amended form, constitute the core of Zermelo's axiomatization. The principles listed in the 1908 are those used in the proof of the well-ordering principle and are certainly not an axiomatization of Cantorian set theory or even an axiomatization as such. But the proof, as Zermelo puts it ,"presupposes no specific theorems of set theory". [1908 p.183] In other words, these principles or "postulates" are a setting out of clear, general principles of Cantorian set theory sufficient for the proof. It is straightforwardly the case that the identification of such general principles facilitated the axiomatization in the 1908a.

(ii) Moore tells us that Zermelo "created an axiomatization of set theory and embedded his proof within it." [1978 p. 326] Now axiomatization is frequently cited as a means of rigourizing proofs within a given mathematical discipline. In fact, Moore opens his 1978 by stating

> Late nineteenth century Europe witnessed an increasing concern with mathematical rigor. One prominent form, though not the only one, which this concern assumed, was the use of the axiomatic method...[p.307]

Hilbert's axiomatization of geometry rigourized the discipline in the sense that many hidden assumptions in proofs were made explicit and gaps in proofs were filled. It is also worth quoting Hilbert in this connection. In his consideration of foundational problems in arithmetic, he stated

> ...we can provide a rigorous and completely satisfying foundation for the notion of number and in fact by a method that I would call *axiomatic*...[1904 p.131]

Now the following question arises: did Zermelo's axiomatization add to the rigour of Zermelo's proof? Well, in the passage from Hilbert we are given a glimpse of the trend leading to the metamathematics that he was to develop in earnest in response to the Brouwerian criticism of classical mathematics. In the notion of 'axiomatic', referred to here by Hilbert, rigour is achieved by, amongst other things, not only making the postulates explicit but also making the underlying logic explicit in pursuence of an effective notion of proof. In other words, rigour rests on the degree and manner of *formalization* of which axiomatization is but one facet. So the answer to the question must be negative since Zermelo did not go very far in the direction of formalization over and above providing axioms. In particular, he provides no explicit information on the details of the underlying logic of his system.

(iii) Although Zermelo's system is by no means a 'formalization' it is a putative codification and precisification of Cantorian set theory. It codifies this theory not only in the sense that it provides the means for the reconstruction of transfinite arithmetic but in that the axioms themselves are taken "from set theory as it is historically given". [1908a p.200] I interpret this as implying that they are true to the Cantorian concept of set.

Some time before Zermelo's work on the well-ordering problem Hilbert had publically recognized, not the inconsistency of the Cantorian notion, but rather the need for its precisification. He writes in his 1904:

G. Cantor sensed the contradictions just mentioned and expressed this awareness by differentiating between "consistent" and "inconsistent" sets. But, since in my opinion he does not provide a precise criterion for this distinction, I must characterize his conception on this point as one that still leaves latitude for *subjective* judgment and therefore affords no objective certainty.

The most remarkable feature of Zermelo's codification and precisification of the Cantorian notion of set is his axiom of separation. The version in his 1908a is, as we shall see, significantly modified. The reason for this modification has nothing to do with his defence of the well-ordering theorem. Rather, in his 1908a, it figures extensively in his development of "the theory of equivalence".

Finally, recall van Heijenoort's contention that "Cantor's definition of set had hardly more to do with the development of set theory than Euclid's definition of point with that of geometry." In one sense Cantor's definition was very important in connection with the axiomatic development of set theory. Put bluntly, this is because it provided the key *heuristics* guiding Zermelo. In particular with respect to the formulation of his the axiom of separation. This point brings us directly to the subject matter of the next chapter.

ZERMELO AND DEFINITE PROPERTIES.

Given that the key and most innovative feature of Zermelo's system is his axiom of separation it is not surprising that it was this axiom which provoked the most intense and sustained criticism. His formulation of the axiom of separation employed the notion of a "definite propositional function" - more commonly referred to in the literature as "definite property". It was this notion that was the focus of the criticism directed at Zermelo's axiomatization. Weyl, Fraenkel, von Neumann and, in particular, Skolem, were the most notable critics.

But despite the extensive criticism, an important question has, in general, been either overlooked, ignored or misunderstood. The question is simply: "What was the motivation behind Zermelo's invocation of definite properties within a separation principle?" In this chapter I propose to shed some light on the conceptual origins of Zermelo's formulation, particularly his use of definite properties. However, little credit can be taken for supplying the *simple* answer to the given question since, as will be seen below, it was clearly supplied by Zermelo.

The axiom of separation is stated by Zermelo as follows:

> Whenever the propositional function P(x) is definite for all elements of a set M, M possesses a subset M(P) containing as elements precisely those elements x of m for which P(x) is true. [1908a p.202]

"Definiteness for propositional functions is defined in terms of "definiteness" of propositions. The propositional function P(x) is definite for a class M if for each element **a** of M the proposition P(**a**) is definite. The proposition P(**a**) is definite if

> the fundamental relations of the domain, by means of the axioms and the universally valid laws of logic, determine without arbitrariness whether it holds or not. [1908a p.201]

By the "fundamental relations of the domain" Zermelo means simply the membership structure.

Recall that in the introduction to his 1908a Zermelo states that Cantor's original definition "requires some restriction" since, in view of the Russell antinomy, "it no longer seems admissable today to assign to an arbitrary logically definable notion a set, or class, as its extension." Zermelo, at this point in his paper, seems to be interpreting Cantor's prescription as a naive comprehension principle, i.e. for every 'condition' there exists a set whose members are all and only those objects satisfying the condition. But this is an anomaly. We have noted Hallett's observation that "nothing like the comprehension principle of so-called 'naive set theory' follows from Cantor's statements." There is no doubt that Zermelo was familiar with Cantor's philosophy of the infinite and thus aware that Cantor was not proposing an unrestricted comprehension axiom. At the same time he was aware, as was Hilbert, that Cantor's ideas needed to be represented in some more *precise* manner. In examining the formulation of the axiom of separation we see that Zermelo is much more in accord with Cantor's thinking than the misleading statements in his introduction tend to suggest.

However, the separation principle may be justifiably construed as a *restricted* version of the comprehension principle. Roughly speaking, instead of a condition collecting together all objects that satisfy it, it collects together all objects *of a given set* that satisfy it. Supplementary axioms such as the power-sets and infinity axioms are needed to supply the initial sets.

Now it is more often than not claimed that it is by means of this restriction that Zermelo hoped to avert the paradoxes, or in Zermelo's terms, "the antinomies discovered so far". This claim is mistaken and indicates a misunderstanding with respect to Zermelo's invocation of definite properties. But this is not to deny the very important role in the solution of these paradoxes played by the aforementioned restriction. The relevant principle underlying Zermelo's approach, with respect to this restriction, is the 'Limitation of Size Hypothesis". In Hallett's excellent account of this topic [see Hallett 1984] its general form is stated thus:

All contradictory collections are too big (in some sense of 'bigness' to be specified), 'contradictory' collection here meaning any collection such that a contradiction can be derived from assuming it to be a set. [p.176]

This principle may be traced back to Cantor's philosophy of the infinite. Specifically, to his partitioning of infinite collections into the "transfinite" and the "absolutely infinite". 'Sets' are either finite or transfinite and together constitute those collections which are 'Cantorian finite', i.e. 'increasable' or 'bounded'. [See Mayberry 1977 and Hallett 1984] The collections that are 'too big' are identified with those that are "absolutely infinite" and further, with the contradictory collections, i.e. those giving rise to the paradoxes. This idea is the heuristic message underlying the first part of Cantor's definition, *viz*: "By a 'set' we understand every collection to a *whole*..." [My italics]

Zermelo's adaption of this principle for the axiom of separation is that *if* a *given* set is not "too big" then a sub-collection separated off from it certainly cannot itself be "too big". But, as Hallett has shown in his 1984, there are severe problems with construing Zermelo's axiomatization as a whole as being underpinned by the limitation of size hypothesis. The following passage serves as a summary:

Part 2 of the book deals primarily with the influence and status of the 'limitation of size' idea, an ideal focus for the question of how far modern set theory is Cantorian set theory, for there is no doubt both that limitation of size was of great significance in the development of the axiomatic theory of sets, and that it stems rather directly from Cantor's metaphysical doctrine of Absolute infinity. Nevertheless, it is important to be more precise about the role limitation of size played (and plays) and not to exaggerate its strength. Part of my purpose is to correct the impression often given (an impression which stems from Fraenkel) that axiomatic set theory avoids the standard paradoxes by choosing axioms which do not create overly large sets, it being assumed that the collection of ordinals, the Russell 'set' of all self-membered sets and the set-theoretic universe itself are all 'too big' in some sense... I argue that not only is it extremely difficult to specify a notion of 'overly large' which successfully embraces all these collections, but that in any case no form of this limitation of size argument can justify the adoption of the impredicative power set axiom. This failure is related to the inadequacy of various attempts to explain the acceptance of the usual axioms by their being true in some informally presented, iterative universe of sets. [1984 pp.xii-xiii]

Allowing that Zermelo's version of the limitation of size hypothesis is sound, i.e. sufficient to avert the paradoxes, then it seems that he has a warrant for formulating the most general separation axiom he deems suitable. For example, if he was content to set aside his reductionist aims, that is, include within his ontology collections that are not 'abstract sets', he might formulate the following version of the separation principle: given a set **a**, for every condition there exists a set whose members are all and only those members of **a** that satisfy the condition. This formulation would allow the employment of those conditions which give rise to the Burali-Forti and Russell paradoxes whilst at the same time blocking those paradoxes.

The point is that if *mere* restriction on size is the means by which Zermelo undertakes to tackle the paradoxes then there is no need to invoke definite properties. *In fact, Zermelo's invocation of definite properties becomes quite unintelligible*!

*

So why did Zermelo invoke definite properties? The simple answer is that they were employed to avert any *semantic* paradoxes that might arise if only a general separation principle, such as the one given above, which we have seen to be underpinned by the limitation of size hypothesis, was employed. It is surprising that this has been largely ignored by commentators. For example, Skolem, Fraenkel and von Neumann, who all devote significant space to discussing definite properties, fail to note that Zermelo invoked the notion of definite properties specifically in order to avert the semantic paradoxes. This is particularly true of recent commentators, e.g. Hallett and G.H. Moore. Moore often acknowledges the importance of this notion in connection with the development of axiomatic set theory, [see e.g. chapter 4.9 1982] and we find in Hallett after some discussion of the notion the comment

..the confusion over definite properties was undoubtedly a serious problem for Zermelo's successors. [1984 p.269]

but neither of them mention the reason Zermelo introduced the notion in the first place. This situation is especially surprising in view of the fact that Zermelo is quite explicit on this point. Furthermore its significance with respect to the Cantorian notion of set and particularly Cantors definition has not been pursued. In fact it was an important facet of Zermelo's precisification of the Cantorian notion!

After stating his axiom of separation Zermelo writes

In the first place, sets may never be *independently defined* by means of the axiom but must always be *separated* as subsets from sets already given; thus contradictory notions such as "the set of all sets" or "the set of all ordinal numbers", and with them the "ultrafinite paradoxes",...are excluded. In the second place, moreover, the defining criterion must always be definite in the sense of our definition...with the result that, from our point of view, all criteria such as "definable by means of a finite number of words", hence the "Richard antinomy" and the "paradox of finite denotation", vanish. [1908a p.202]

The simple answer therefore, to the question posed above, is that Zermelo's invocation of definite properties was aimed at averting the semantic paradoxes. But this simple answer raises two further problems. First, why did Zermelo concern himself with the semantic paradoxes? Second, how were definite properties supposed to deal with the semantic paradoxes?

The semantic paradoxes that were to trouble Zermelo began to make their appearance in 1905 in separate and independent papers by Richard and König. Richard had read that König, at the 1904 Heidelberg conference, had formulated an argument based on the theory of transfinite ordinal theory showing the well-ordering principle to lead to contradictions. He had also learned of Zermelo's counterargument and subsequent proof of the theorem. But in Richard's view, as he puts it "It it not necessary to go so far as the theory of ordinal numbers to find such contradictions." Richard devised his paradox with reference only to the continuum. This strategy was pursued by König in his 1905 and 1906 as part of his continuing effort to show that the well-ordering principle was invalid.

In his 1905, König's argument took the following form: Let us suppose, in accordance with the well-ordering principle, that the continuum is well-ordered. Consider the subcollection $\mathbf{M}$ consisting of those real numbers satisfying the condition that they not be finitely definable. Since $\mathbf{M}$ is well-ordered it has a least member. Call it $\mathbf{a}$. But a finite prescription, namely: "least member of the collection of real numbers not finitely definable" uniquely picks out $\mathbf{a}$. Hence we have both $\mathbf{a}\epsilon\mathbf{M}$ and not $\mathbf{a}\epsilon\mathbf{M}$. König took this to be a refutation of the well-ordering principle.

But the more fundamental lesson König states at the outset of his paper. It is that

> the word "set" is being used indiscriminitely for completely different notions and that this is the source of the apparent paradoxes of this young branch of science, that, moreover, set theory itself can no more dispense with axiomatic assumptions than can any other exact science and that these assumptions, just as in other disciplines, are subject to a certain arbitrariness...[1905 p.144]

So if not Zermelo, then at least König took the paradoxes as a spur towards axiomatization. Incidentally, like Zermelo, König was basically a conservative with respect to Cantorianism. In his concluding paragraph he states that his "fragmentary remarks"

> insofar as they are correct, they only throw a new light on the great value of what Cantor's genius created, despite their partially oppositionist character. The opposition is directed only against certain of cantor's conjectures; the content of the theorems he proved remains completely intact. I remark, finally, that the distinction here drawn between "set" and "class" completely resolves the paradoxes cited ("set of all sets", and so forth). [1905 p.149]

There were some commentators in this period, i.e. 1904-1908, who denied that the semantic paradoxes were efficacious so far as providing an argument against the well-ordering principle was concerned. Whilst Richard and Poincaré concentrated on the nature of the definitions within the construction of these paradoxes Peano simply denied that they were even relevent to mathematics. Peano claimed that Richard's paradox belonged linguistics and not to mathematics. Zermelo, however, took them seriously as possible threats to the well-ordering theorem and indeed considered it necessary to take active steps to avert them within his axiomatization. These steps, as we shall see, are directly in accord with Zermelo's precisification of the Cantorian notion of set.

The question of how definite properties were supposed to tackle the semantic paradoxes is made more difficult by the fact that Zermelo offers no clear explication of his notion of definite property and no demonstration illustrating the manner in which they avert the semantic paradoxes. But I contend that the following reconstruction is essentially correct. To begin with, note that the notion is relativized to models. Hence the notion of truth in the ensuing discussion is roughly akin to the informal idea behind the model theorists 'truth in a model', i.e. without the trappings of a formal semantics. Moreover, the notion is further relativized to 'sets' in the sense of the model in question. In other words, if we assert that a property is definite then this is always relative to a pair $\langle A, \mathbf{M} \rangle$ where A is a model, $\mathbf{M} \in A$ and $\mathbf{M}$ is a 'set'.

Now a property P(x) is definite for a set $\mathbf{M}$ if for each element $\underline{a}$ of $\mathbf{M}$, P($\underline{a}$) is exclusively true or false. Thus the property P(x) induces a partition of $\mathbf{M}$ into two disjoint collections. One containing all and only those elements of $\mathbf{M}$ satisfying P(x); the other those not satisfying P(x) and we understand to imply that they satisfy not-P(x). As an example, let $\mathbf{M}$ be the set of real numbers and let P(x) be "x is definable by means of a finite number of words". Let $\underline{a}$ be the least real number *not* definable by means of a finite number of words. Then it is straightforward that P($\underline{a}$) is not exclusively true or false. Thus the property in question is not definite.

In his 1908 Zermelo states his separation principle as follows:

> All elements of a set **M** that have a property P well-defined for every single element are the elements of another set, **M**(P), a "subset" of **M**. [1908 p.183]

The account of the axiom of separation in the 1908a paper is a modified and more detailed version of that in the 1908, although he seemed to have had a similar idea in mind in both accounts. But why does he provide a more detailed account in the later paper? I have argued in the previous chapter that Zermelo considered the 1908 paper a sufficient reply to the critics of his 1904 proof of the well-ordering principle and that the axiomatization in the following paper was not motivated by the need to answer these critics. This also holds for Richard and König with respect to the semantic paradoxes. In the 1908a there is no application of the modified version in connection with the semantic paradoxes. In fact, Zermelo makes no mention of them other than in the passage quoted above. In any case, why not provide the expanded account in the 1908 - they were written at about the same time?

On reading the second (and longest) part of the 1908a, i.e. the development of fundamental cardinal theory from the axioms, the reason Zermelo gives a more detailed account of separation in this later paper becomes manifest. After giving his account of definite properties Zermelo states

> Thus the question whether $\mathbf{a} \in \underline{b}$ or not is always definite, as is the question whether $\mathbf{M} \leq \mathbf{N}$ or not. [p.201]

In the development of the cardinal theory, Zermelo builds up definite properties from these basic ones applying the axiom of separation to construct the fine structure of the theory. This is here the key application of the separation principle and for which the expanded account is added for clarificatory purposes.

Zermelo's general idea embodied in the axiom of separation may be located in the Cantorian notion of set and in particular in the definition from Cantor's 1895. In that definition we also found the qualification 'definite'. Fraenkel comments on the qualification 'definite' in Cantor's characterization as follows

> [it] .. expresses that, given a set s, it should be intrinsically settled for any possible object x whether x is a member of s or not. Here the addition "intrinsically" stresses that the intention is not to actual decidability with the present (or with any future) resources of experience or science; a definition which intrinsically settles the matter, such as the definition of "transcendental" in the case of the set of all transcendental numbers, is sufficient. To be sure, we thus essentially use the Aristotelian *principle of the excluded middle* which guarantees that for a given object there is no case additional to those of belonging or not belonging to the set in question. [1968 p.10]

Thus it follows that the condition "x is not a member of itself" is not definite in Cantor's sense. Cantor, like Zermelo after him, is invoking the qualification "definiteness" as an auxiliary to the limitation of size hypothesis as a means of averting paradoxes. An important difference is that Cantor's conditions apply globally and thus a condition is definite or not *per se*; whilst Zermelo's approach is strictly speaking local and in principle a condition may be definite over one class although not so over some other.

At the time Cantor was developing his theories the relevant semantical paradoxes had not yet emerged. Cantor's concern was with the set-theoretical paradoxes i.e. "ultrafinite paradoxes" such as that of Burali-Forti's. This indicates a concentration on limitation of size as a sufficient strategy for averting paradoxes. As we have seen, Zermelo's local approach reflects the reliance on the limitation of size hypothesis only so far as the ultrafinite paradoxes are concerned. The semantic paradoxes were averted by the invocation of definite propositional functions.

PROBLEMS INHERENT IN ZERMELO'S ACCOUNT OF DEFINITE PROPERTIES.

After the publication of Zermelo' axiomatization there ensued considerable criticism, variously motivated, of his account of definite properties. In the widespread criticism of Zermelo's definite properties the fact that Zermelo was largely successful in achieving his instrumentalistic aspirations is often ignored. This should alert us to the need to reexamine the professed motivation for some of this criticism. At the same time Zermelo's prescription for definiteness is certainly problematic and in certain respects flawed. In this chapter I discuss some of the problematic aspects of this prescription. A critique of Zermelo's account is rewarding in that it reveals valuable details of Zermelo's overall approach, highlights the nature of Skolem's reformulation of Zermelo's notion of definite property and provides material for an evaluation of that reformulation.

Commentators, including Skolem, criticize Zermelo on the grounds that his notion is too 'vague' or 'imprecise'. What is not pointed out, and which is perhaps its principal defect, is the *circularity* of his explication. The explication is circular since definiteness occurs in the statement of the axioms and the axioms determine what is definite. Or put in another way, which propositional functions are definite depends on the membership structure. This in turn, depends on the sets generated by the axiom of separation. But which sets are generated by this axiom depends on which propositional functions are definite. There is an indication here that definite properties should be characterized in some manner independent of the axioms.

On reading Zermelo's explication of definite property we seem to be in vertiginous suspension between the syntactic and semantic modes of interpretation. Zermelo, in characterizing definite propositional functions makes no explicit statement as to whether they are to be understood as syntactic entities, properties or otherwise. In all probability this omission occurred simply because Zermelo did not perceive the import of the distinction between syntax and semantics. This failure of logical insight was not peculiar to Zermelo. On the contrary, it was shared by the majority of those working in the first quarter of the century. Whilst it is true that Frege had been aware of the need to strictly differentiate between syntax and semantics his ideas were not immediately influential on this matter. Moreover, vagueness with respect to this distinction was apparent not only among mathematicians such as Zermelo but also among logicians. For example, Russell, as Quine informs us

> used "propositional function" to refer both to attributes and open sentences or predicates. [1963 p.19]

and furthermore

> Russell's own exposition simply blurred the distinction between the abstractive expression (or even the open sentence) and the propositional function (or attribute or relation)...[1963 p.245]

Russell, incidentally, with the benefit of some fifty years hindsight, writes the following of the notion of propositional function employed in the formulation of his ramified type theory

> A propositional function is an expression containing a variable and becoming a proposition as soon as a value is assigned to the variable. For example, "x is a man" is a propositional function. If, in place of x, we put Socrates or Plato or anybody else, we get a proposition. We can replace x by something that is not a man and we still get a

proposition, though in this case a false one. A propositional function is nothing but an expression. It does not, by itself, represent anything. [1959 p. 53]

Zermelo states his separation principle as a single axiom. A feature of the axiom is quantification over propositional functions. This higher-order quantification invites an interpretation of definite propositional functions as 'properties' or 'attributes' rather than syntactic objects. According to Hallett there is some evidence for this interpretation to be found in Zermelo's later writings. Hallett writes that

In his [1929], Zermelo attempts to explain his notion of 'definite property'. He proposes an axiomatization of definite properties, or, more strictly, an axiomatization of the concept of 'definiteness' for properties. This approach, he tells us, goes back to his [1908a]; it is '... the method which I myself had in view, though I did not expressly say so, and which was applied in the reasoning of the work mentioned' ([1929], p. 340). 'Definiteness' is now treated as a predicate under which properties (or relations etc.) fall, and, importantly, the axioms proposed for definiteness involve quantification over the properties. The permissibility of quantification over properties is confirmed in Zermelo [1930]. There he states what appears to be a second-order version of the axiom of separation: 'Every propositional function P(x) separates from each set M a subset P(M) containing all elements x for which P(x) is true' ([1930)], p.30). [1984 p.268]

Hallett goes on to say that the evidence in the 1908a for the interpretation in question "is simply not clear". Moreover,

There, separation is not stated in a form which suggests second-order quantification, and Zermelo largely avoids the term 'property' [p.268]

Whilst I agree with Hallett that Zermelo's 1908a is not clear on this point, I am less agreeable to Hallett's contention that the form of the statement of the axiom of separation does not suggest second-order logic. The quantification in the axiom is over propositional functions and thus is manifestly a higher-order logic. What isn't clear is the nature of these higher-order entities over which he is quantifying. However, it is the case that insofar as the statement of the separation principle in the 1908 paper is taken as a guide, then it serves as a warrant to construe Zermelo's definite propositional functions as properties.

Two further remarks arising from the above discussion. First, it would be surprising if Zermelo, at this stage in the development of logic, did not intend a higher-order system. Both Frege's *Begriffsschrift* [1879] and Russell's theory of types [1908] contain a first-order logic as a subsystem, but this subsystem was not singled out by them for special purposes. In fact, it is generally held that it was only in 1915, in a paper of Löwenheim's, that the first-order fragment of logical systems was focused upon. But even here we must be cautious. For example, Moore states that Löwenheim

> became the first logician to separate first-order logic clearly from second-order logic, and to acknowledge that first-order logic deserved to be studied in its own right. [1980 p.100]

But Löwenheim did not single out the first-order part of logic *qua system of logic* so much as point out a particular property of sentences which only displayed quantification over individuals.

Second, we may, of course, interpret the 'P' in his statement of the axiom as a schematic symbol rather than intended as a variable proper. After all, in stating the axioms of ZF, for example, such a schematic symbol is employed without intending a second-order formalization. But there is no evidence for this somewhat anachronistic interpretation.

I now turn to the question of construing Zermelo's notion of definite property as a linguistic entity. Recall that in explicating his notion Zermelo refers to "a propositional function P(x), in which the variable term ranges over all individuals of a class M,..." Now a variable is a syntactic entity. It may, following Frege, 'indicate a range' but this informs us as to how we deal with it in our semantic theory. It is not a semantic entity *per se*. Now since a variable is part of the constitution of a propositional function it is not unreasonable to conclude that a propositional function is itself a syntactic entity - more specifically, an open formula.

If we interpret definite propositional functions as open formulas then this naturally leads to an enquiry into the underlying language in which they are embedded. This is an important question so far as the foundational viability of Zermelo's axiomatization is concerned because the axiom of separation is the key tool to be employed to reconstruct extant mathematical disciplines such as classical analysis, especially its fine structure and, as is made evident in the second part of Zermelo's 1908a, transfinite arithmetic. Since those propositional functions which are definite are part of the stock of propositional functions in general it seems plausible that the extent of this part may vary with a variation in the extent of the general collection of propositional functions which itself depends on the underlying language.

Before proceeding with the enquiry into the underlying language of definite propositional functions I first consider a consequence of Zermelo's characterization of them which apparently presents a difficulty for their construal as linguistic entities. Let P(x) be a propositional function and **M** a class. Recall that according to Zermelo's prescription (which he presents as relativized to a model) P(x) is definite if it is definite for all members of **M**. That is, if for all members **a** of **M**, P(**a**) is definite. Zermelo unequivocally states that "whether **a**ϵ**b** or not is always definite". If we are to interpret propositional functions to be linguistic entities then this implies that the underlying language includes, for example, an individual constant for each member of **M**. However, since the cardinality of **M** maybe arbitrarily high it follows that this is also the case for the cardinality of the underlying language.

This consequence might be taken as militating against the interpretation of propositional functions as open formulas. But Zermelo had very liberal views on languages. For example, from Zermelo's point of view even infinitary languages were not problematic. Note first, that an espousal of infinitary languages by Zermelo, viewed in its historical context, cannot be considered a radical position. In fact the use of infinitely long expressions within systems of logic was quite common in the late nineteenth and early twentieth century. For example, Peirce, Schröder and Löwenheim all employed them - the latter even allowing expressions with infinite strings of quantifiers.

Having previously pointed out that Hilbert's ideas were a great influence on Zermelo, at least in the first decade of the century, it is worthwile here to quote the following passage concerning Hilbert and infinitary languages.

> At the Third International Congress of Mathematics, held at Heidelberg, Hilbert analysed the foundations of logic and of the real numbers. To secure these foundations properly and to circumvent the set-theoretic paradoxes, he insisted that the laws of logic and some of those for arithmetic must be developed simultaneously. Above all, he considered such paradoxes to indicate that traditional logic had failed to fulfill the rigorous demands that set theory now imposed on it. In the course of outlining a logical theory for the positive integers, Hilbert employed both infinite conjunctions...and disjunctions... Since he did not cite either Peirce or Schröder, it appears that Hilbert independently formulated this method of defining quantifiers with a fixed domain...Two decades later he came to employ a version of the Axiom of Choice, rather than infinite expressions, in order to define quantifiers. [Moore 1980 p.99]

In contrast to Hilbert, Zermelo did not come to eschew infinitary logic. In his replies to Skolem's criticisms of his axiomatization Zermelo argued for a logic incorporating arbitrarily long conjunctions and disjunctions. These were arbitrarily long in the sense that they might be indexed by any given ordinal. [See Zermelo 1929, 1930, 1932]

(It might be argued that Zermelo's espousal of infinitary logic was simply a result of a conflation between syntax and semantics. However, he did continue to advocate an infinitary logic as late as 1932 by which time Gödel's work, beginning with his completeness theorem for first-order logic [1930], following an open problem suggested by Hilbert and Ackermann in their 1928, had done much to clarify ideas with respect to the distinction between syntax and semantics. Zermelo was familiar with Gödel's work and in fact he had discussed it with

Gödel through an exchange of letters. But there is some suggestion that Zermelo was not entirely clear on Gödel's ideas. Feferman writes:

> Gödel's work...drew criticism from various quarters, which was invariably due to confusions about the necessary distinctions involved, such as that between the notions of truth and proof. In fact, the famous set-theorist Ernst Zermelo interpreted these concepts in such a way as to arrive at a flat contradiction with Gödel's results. In correspondence during 1931 Gödel took pains to explain his work to Zermelo, apparently without success. [1986 p.6]

This, of course, is not a direct comment on the syntax/semantics distinction but it does add weight to Moore's forthright opinion that in his 1932 Zermelo "did not distinguish clearly enough between his syntax and semantics." [1980 p.126])

Given that Zermelo's 1908a was written some ten or more years before the advent of Hilbert's full-blown metamathematics and that he was ambiguous with respect to the distinction between syntax and semantics it is not surprising that he does not make explicit the underlying language of his definite propositional functions. However, as we shall see, it is not difficult to give a plausible account of it. Not difficult, that is, with the exception of one important syntactic category, namely: the logical symbols.

The underlying language of Zermelo's definite propositional functions excluding the logical component comprises of a single *primitive*, i.e. 'ϵ' to be read informally as the membership relation. This is directly in line with Cantor's idea of a set as a purely extensional object, i.e. a collection completely determined by its members - all other properties having been 'abstracted' away, so to speak. Thus 'membership' is the single primitive notion of Cantorian set theory and Zermelo's axiom system is a theory of membership essentially requiring just the one binary relation symbol. Supporting the above assertions about the underlying language are the following considerations.

The axioms as stated by Zermelo, with the possible exception of the axiom of the axiom of separation, refer only to the membership relation or relations and set theoretical operations compounded from this relation, e.g. 'subset' and 'intersection'. From this fact we can conclude that membership is the only primitive non-logical notion constitutive of definite propositional functions. If not, then we should expect them to be axiomatized, i.e. there should be further axioms referring to them. But there are none. Moreover, Zermelo is clear that the "fundamental relations over the domain " are of the form $\underline{a} \epsilon \underline{b}$. Another point is that in the application of his axioms in part two where the notion of definite property is extensively employed it is clear that they are all constructs starting from the 'fundamental relations'. (Incidentally, also from this application, it is clear that Zermelo presumes that definite propositional functions are closed under the logical operations of negation, conjunction and disjunction. In so far the last two are infinitary then we have a warrant to extend the closure to universal and existential quantification. A further point worth mentioning here is that although, strictly speaking, Zermelo gives a local prescription for definiteness; in practice it is global, i.e. all the definite propositional functions that he employs are apparently definite relative to *any* class M. This last particular is perhaps is an indicator that a formulation of definite properties independent of particular models and classes would be viable.)

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In Zermelo's prescription for the definiteness of assertions there are three parameters: the "fundamental relations of the domain"; the axioms; the "universally valid laws of logic". If the first of these "determines without arbitrariness" whether a particular assertion "holds or not" then that assertion is definite. But this determination is "by means of the axioms and universally valid laws of logic". Judging from the prescribed role of these last two parameters it is tempting to conclude that the collection of definite assertions are to be identified with the closure of the axioms under logical consequence. Thus, in general, this collection varies in extent according to which principles count as logical. (Note that from Zermelo's point of view these these principles could involve notions such as 'truth' and 'definability' hence the worry over the introduction of semantic paradoxes.) Another point here is that any assertion independent of the axioms cannot be definite. However, if we interpret his prescription in this way the part played by the first parameter is completely undermined.

Recall that the interpretation offered in chapter 4 above was that an assertion was definite if it was unambiguously true or false relative to the given model. The role of the axioms and logic serve to circumscribe the models. What I mean by the logic circumscribing the models is simply that if some principle is accepted as a genuine logical principle then a structure not satisfying it will not be considered merely as an interpretation which does not satisfy a particular axiom but rather not even admissable as an interpretation. (Of course, in contemporary model theory considerations such as these are subsumed under the arrangements made to set up the formal semantics. But Zermelo was working some time before the advent of formal semantics.) Furthermore, it follows from Zermelo's prescription that whether a particular assertion is definite in general depends on whether a particular principle fails to hold. However, if it is accepted as a logical principle it *must* hold.

Now the central point I wish to emphasize here is that whether you interpret the determination of definite assertions "by means of the axioms and universally valid laws of logic" in either of the ways suggested in the last two paragraphs definiteness depends on which principles are accepted as "universally valid laws of logic". Thus the foundational credentials of his system depends on them since definiteness is the warrant for the separation operation. However, Zermelo offers no explicit details as to the underlying logic of his system. So this omission contributes a further degree of imprecision to Zermelo's notion of definite property.

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I have intimated that a characterization of definiteness such that the notion is independent of models, classes, the underlying logic or set of axioms would resolve *some* of the problems brought about by Zermelo's prescription. Was such an account offered by Skolem?

SKOLEM'S REFORMULATION OF ZERMELO'S NOTION OF DEFINITE PROPERTY.

(i) Introduction.

Skolem's reformulation of Zermelo's notion of definite property gains immence significance from the fact that it constituted a key step leading to the general acceptance by mathematicians and mathematical logicians over the last half-century, that set theory, upon formalization, is to be framed as a first-order system. Furthermore, Skolem's first-orderization of set theory anticipated the general movement towards first-orderization of mathematics. This movement, which gained momentum in the 1930's, underpinned by Gödel's completeness theorem (1930) and Tarski's formal semantics (1936), is in fact parasitic on the first-orderization of set theory.

But the trend towards first-orderization was not, and is not, left unchallenged. [For details of the substantial present-day challenge see e.g. Barwise/Feferman 1985.] Zermelo, in particular, opposed this trend and took issue with Skolem and von Neumann on this aspect of their respective reformulations of his system. As Moore informs us

> It was Zermelo, perhaps influenced by Hilbert, who...argued that first-order logic should not suffice for mathematics and especially for set theory. [1982 p.267]

Hilbert's influence in this particular direction is perhaps surprising. By the 1920's Hilbert had begun to develop his mature foundational programme, namely: his metamathematics. Loosely speaking, this involved framing mathematics within fragments of primitive arithmetic. [See Giaquinto 1983] So this does not look, at least from a contemporary perspective, as if Hilbert at that stage would be a proponent of second-order logic. The central exposition of Hilbert's metamathematics is to be found in his paper entitled "On The Infinite"; published in 1925. The chronology is to be noted here, for Moore cites as support for his interesting contention the following quotation dating from 1928:

> As soon as the object of investigation becomes the foundation of...mathematical theories, as soon as we want to determine in what relation the theory stands to logic and to what extent it can be obtained from purely logical operations and concepts, then second-order logic is essential. [Hilbert and Ackermann 1928 p.86 Moore's translation.]

In his 1922 Skolem shows remarkable insight and prescience on matters regarding the logical and foundational aspects of Zermelo's axiomatization. Although he suggests a means to render Zermelo's system more precise at the same time his attitude towards the foundational viability of axiomatic set theory is essentially negative. In the concluding remarks of his paper he confesses:

> I believe that it was...clear that axiomatization in terms of sets was not a satisfactory ultimate foundation of mathematics, that mathematics would, for the most part, not be very much concerned with it. But in recent times I have seen to my surprise that so many mathematicians think that these axioms of set theory provide the ideal foundation for mathematics; therefore it seemed to me that the time had come to publish a critique. [pp.300-301]

In fact, throughout much of his career, Skolem's attitude towards the foundational viability of set theory was negative. As Fenstad informs us

Skolem...did not have much confidence in set theory as a foundation for "real" mathematics, and he was extremely doubtful about the transfinite powers and non-constructive modes of reasoning of set theoretical mathematics. His own preferences are better represented by the important 1923 paper. Here Skolem tries to build up elementary arithmetic without applying the unrestricted quantifiers "for all" and "there exists" to the infinite completed totality of natural numbers. Historically this is perhaps "a first" paper in the theory of recursive arithmetic. [1967 p.12]

In his 1922 Skolem's scepticism is directed against the foundational adequacy of an *axiomatic* theory of sets. Later in his career this was extended to set theory in general. It is important to emphasize the distinction between the two attitudes. The latter reflects upon the foundational adequacy of the notion of set whilst the former upon the axiomatic method. It is a perfectly coherent attitude to take set theoretical notions as being foundationally adequate, whilst at the same time maintaining that no axiomatic theory of sets fulfills, or even in principle may adequately fulfill this foundational role. Given that set theoretical notions are foundationally adequate then Skolem's discussion of his first remark may serve to underpin this attitude. Specifically his contention that

If we adopt Zermelo's axiomatization, we must, strictly speaking, have a general notion of domains in order to be able to provide a foundation for set theory. The entire content of this theory is, after all, as follows: for every domain in which the axioms hold, the further theorems of set theory also hold. But clearly it it is somehow circular to reduce the notion of set to a general notion of domain. [1922 p.292]

Note that the above passage makes it evident that Skolem understood Zermelo's axiomatization to be an implicit definition. Moreover it seems that this was Skolem's general view of the status of axiom systems. In Part I it was discussed how the implicit definition thesis goes hand in hand with a relativistic view of mathematical notions. Now later on his career Skolem, did, in fact, come to adopt an explicit relativistic view of set theory together with a *positive* attitude towards the axiomatic method as a means for its foundation. In a paper of 1941 he writes

> ...that axiomatization should lead to relativism is a fact sometimes considered to be a weak point of the axiomatic method. But without reason. The analysis of mathematical thought, the fixing of its fundamental hypotheses and modes of reasoning can only be an advantage for science. It is not a weakness of a scientific method that it doesn't yield the impossible. But it appears that most mathematicians are terrified that the absolute theory of sets should turn out to be an impossibility. [1941 p.470. Translated in Benacerraf 1985.]

Skolem's relativism, according to Hao Wang, had a tendency to shade over into formalism. In a paper of 1958, as Hao Wang puts it, Skolem

> ... proposes to present "a relativist conception of the fundamental notions of mathematics. It seems to me that this is clearer than the absolutist and platonist conception which dominates classical mathematics." A proposition is true if it is formally proved in a formal system. In simple cases such as recursive arithmetic, the intuition of induction is sufficient to guide the construction of the formal system and to assure consistency. "In the other case, one may accept an opportunist view. My point of view is then that one must use formal systems for the development of mathematical ideas." After listing diverse formal systems of set theory, Skolem asserts: "I do not understand why most mathematicians and logicians do not appear to be satisfied with this notion of sets as defined by a formal system, but contrariwise speak of the limitations of the axiomatic method. Of course this notion of set has a relative character, because it depends on the chosen formal systems. But if this system is chosen suitably, we can none the less develop mathematics on the basis." [1970 p.40]

Skolem, as is made evident by his fourth remark, subscribed to the central tenet of the mathematical approach, namely: the generation of Cantorian transfinite arithmetic is a necessary feature of an axiomatic theory of sets. As most workers within the mathematical approach, Skolem displays a strong instrumentalistic streak. He seems to have perceived the general enterprise of axiomatizing set theory as undertaken with an instrumentalistic spirit. This emerges, for instance, in the following passage:

> Set theory in its original version led, as we know, to certain contradictions (antinomies), and no one has yet succeeded in giving a clarification of them that has won general acceptance. In view of this threat to set theory, attempts have been made to develop that theory by means of certain fundamental assumptions, or axioms, in such a way that the part presumed correct and useful would remain provable while the contradictions would be avoided. [1922 p.291]

Note also that in this passage Skolem professes the standard view of the reason for axiomatizing set theory, *viz*: the antinomies. Are we to conclude from this that Skolem understood Cantor's notion of set to incorporate the naive comprehension principle? This, as in Zermelo's case is puzzling. It is unlikely that Skolem is *merely* taking Zermelo's statement on this matter in the introduction to his 1908a at face value. Skolem was certainly familiar with Cantor's transfinite arithmetic and since he studied in Göttingen in the year 1915-16 he was likely to be familiar with Cantor's philosophy of the infinite if not his original writings. On the other hand in his mention of "clarification" he is most probably referring to the attempts of Russell and Poincaré to analyse the contradictions in terms of the notion of 'impredicativity'. (Some evidence for this is provided by Skolem's reference to Russell and Poincaré's criticisms of the use of impredicative definitions in his fifth remark.) So this indicates an affirmative response to the above question. In that case, since Cantorian set theory is interpreted as being contradictory, the claim that Skolem's system is a putative axiomatization of Cantorian set theory needs to be qualified.

Incidentally, if Skolem, in the above passage, is intimating that a successful conceptual analysis or 'clarification' obviates the need for an axiomatization then he is mistaken. On the contrary, it may serve as the spur or starting point for one. The formulation of Russell's theory of types is an illuminating example. As Chihara points out

> Having agreed with Poincaré about the source of the paradoxes - the supposed source being, in each instance, a violation of the vicious-circle principle - one might think that this would have satisfied Russell's desire for a solution to the paradoxes. This was not so: for Russell, the vicious-circle principle was "purely negative in its scope"; he felt that an adequate solution to the paradoxes must provide a *positive* theory which would "exclude" totalities in accordance with the vicious-circle principle. [1973 p.10]

The theory of types was the "positive" theory developed by Russell. I turn now to Skolem's precisification of Zermelo's putative axiomatization of Cantorian set theory.

(ii) Skolem's characterization of definite properties.

Skolem begins his second remark with

A very deficient point in Zermelo is the notion "definite proposition". Probably no one will find Zermelo's explanation of it satisfactory. So far as I know, no one has attempted to give a strict formulation of this notion; this is very strange, since it can be done quite easily and, moreover, in a very natural way that immediately suggests itself. [1922 p.292]

He then goes on, in order to explain this formulation and "also with a view to later considerations", to list "the five basic operations of mathematical logic" as conjunction, disjunction, negation, universal quantification and existential quantification. After which he proceeds to reformulate Zermelo's notion of definite propositional functions as follows:

By a definite proposition we now mean a finite expression constructed from elementary propositions of the form $\underline{a}\epsilon\underline{b}$ *or* $\underline{a}=\underline{b}$ *by means of the five operations mentioned.* This is a completely clear notion and one that is sufficiently comprehensive to permit us to carry out all ordinary set-theoretic proofs. [p.292-3]

At this point Skolem has not specified explicitly whether his new version of Zermelo's system is intended to be in any way different from the original except in respect of a new reading of definite properties. In particular, there is no such specification in connection with the underlying logic. It is in the course of making his third remark that Skolem explicitly completes the transformation of Zermelo's axiomatization into a first-order system. He observes that

Axiom III (axiom of separation) can be replaced by an infinite sequence of simpler axiom's - which like the rest of Zermelo's axioms, are first-order propositions in the sense of Löwenheim - containing the two binary relations ϵ and =. [p.295]

A definite proposition is thus a *syntactic* entity. Skolem's formulation identifies definite propositions with first-order formulas of a language whose only non-logical symbol, with perhaps the exception of constants, is a binary predicate for the formalization of the membership relation. Furthermore, if we assume a primitive recursive language, Skolem's notion is *effective*. (There has been a discrepancy in the interpretation of Skolem's characterization. For example, Hallett writes "Skolem proposed a first-order formulation, 'definite property' thus being replaced by 'predicate of the first-order language'" [1984 p.269] In the context of 'separation' Hallett's interpretation seems intuitively correct and Skolem's usage in the footnote on p.297 *does* suggest this interpretation. On the other hand, from Skolem's discussion of the replacement principle it appears that a definite property may have at least two variables. The source of the ambiguity may be traced to the tradition in logic within which Skolem was working - see the discussion on 'Skolem and Completeness' in chapter 6)

Skolem's notion is clearly independent of any model and class. A proposition is definite regardless of what holds of "fundamental relations of the domain" and to which particular set in the domain we are applying the axiom of separation. In practice, i.e. the actual use of the axiom of separation by Zermelo in his 1908a, this was also the case. But Skolem's characterization forthrightly *ensures* that definiteness is not relativized to models. It is also straightforward that Skolem's notion is independent both of the axioms and underlying logic of the system. Thus, for example, adding extra axioms to the system, e.g. the axiom of foundation; or removing existing ones, does not alter the extension of the class of definite properties.

Trivially, in that Skolem's definite properties are closed under certain given logical connectives and quantifiers, they are not independent of the logic. Rather, they are independent of the underlying logic of the system in the non-trivial sense that they are independent of the rules of inference. But Skolem does not list these rules explicitly. Like Zermelo, Skolem did not present a formal system in the Hilbertian sense. It is, however, a much better approximation to such a system than is Zermelo's. Nevertheless, although Skolem is clear on the fact that he is presenting a first-order system, we are not directly informed to exactly *which* first-order system. Just as Zermelo in his reference to the "universally valid laws of logic" Skolem writes as if there is some common 'folklore' logic to which we are to assume he is alluding. There is, however, in his discussion of Löwenheim's 1915 a hint that he had in mind a variation of the logic of Schröder's 1895 that excludes nondenumerable conjunction and disjunction. [See Skolem 1922 p.293]

(iii) Why did Skolem reformulate Zermelo's axioms as a first-order system?

In so far as Skolem sets out to provide a "A definition, much to be desired, that makes Zermelo's notion 'definite proposition' precise" he certainly achieves his purpose. Although given some the criticisms he makes of axiomatic set theory in his ensuing remarks one begins to wonder why he deemed the exercise "much to be desired". It is sometimes overlooked that Zermelo is successful in the sense that he provides in the second part of his 1908a a forceful demonstration that his system serves as a foundation for a considerable portion of set theory in particular and extant mathematics in general. Moreover, this demonstration employs the notion of definite property formulated in the first part of the paper. Given Zermelo's apparent success; why did Skolem need to make the notion any more precise? Was there something specific in informal set theory or extant mathematics for which Zermelo's version of the axiom of separation was inadequate? If so, Skolem doesn't mention it.

The point of Skolem's third remark is that

...*axiomatizing set theory leads to a relativity of set-theoretic notions, and this relativity is inseparably bound up with every thoroughgoing axiomatization.* [p.296]

Skolem proves a version of what is now known as the Löwenheim-Skolem theorem. He demonstrates that if an infinite sequence of first-order propositions, indexed by the positive integers, are consistent then those propositions are all satisfied in some countable domain. This theorem is an extension on the work Skolem had done on Löwenheim's 1915 in his 1920 which is based on his proof of a normal form theorem for first-order propositions. Skolem's theorem is an example of the so-called 'first-order diseases' and is forthrightly a result concerning first-order theories. Now since Skolem's reformulation of definite property, as he is fully aware, turns Zermelo's axiomatization into a first-order system, and hence susceptible to these 'diseases', *what reasons did Skolem have, if he is questioning the viability of an axiomatic foundation for set theory* in general, *for concentrating on, or even considering a first-order formulation*?

Skolem's professed aim was to render Zermelo's notion precise; and he indeed formulates an effective notion. Skolem was certainly interested in effectiveness and considered it an important aspect of mathematical concepts. This interest is part and parcel of his work on 'decision' problems in logic and what came to be known as primitive recursive arithmetic. We find, as an example of his thinking in this connection, the following comment at the close of his eighth remark

> ...most mathematicians want mathematics to deal, ultimately, with performable computing operations and not to consist of formal propositions about objects called this and that. [p.300]

This interest in effectiveness informs us as to why Skolem was prompted to formulate a syntactic account of definite property. But Skolem could have, for instance, employed the equally effective notion 'formula of the second-order theory of membership'. In fact, Weyl in his 1917 formulated an effective notion of definite property which was identical to Skolem's with the exception that he did not exclude higher-order quantification.

Of course, I am here presuming that Skolem's characterization of definite property *is* 'formula of the first-order theory of membership'. This is certainly how it is generally interpreted; and as such was the blueprint for ZF. But to be precise - in his initial characterization there is nothing to preclude us reading his prescription as the unqualified 'formula of the theory of membership.' It is only in the course of making his third remark that he states that Zermelo's axiom "can" be stated as a schema comprising first-order instances and then states "we may then conclude..." and the conclusion is that the reformulated axioms have a model with domain the natural numbers. In the remainder of his paper Skolem apparently retains this version of set theory but he makes no explicit comment on the matter.

Now the cynic could argue that Skolem transformed Zermelo's axioms into a first-order system merely in order to facilitate his criticisms, which he apparently deems *sufficient*, of the programme of providing an axiomatic foundation for set theory. But at the same time it must be said that it seems unlikely that it was not clear to Skolem that his criticisms did not apply to axiomatic set theory in general. Put another way, are there some positive factors which inclined Skolem to concentrate on the first-order version and as such obviate the view that Skolem had set up a 'straw man' which in any case was not the appropriate target for a *general* case against the above-mentioned programme? I claim there are such positive factors.

To bring these positive factors into focus it must be emphasized that Skolem's criticisms were made with Zermelo's *particular* axiomatization and 1908a paper in mind. In other words, although Skolem was aiming to criticise the idea of an axiomatic foundation for set theory in general the only *visible* target was Zermelo's system and his discussion and application of it in the aforementioned paper. In fact Skolem says as much in the opening paragraphs of his paper - he writes

> Until now, so far as I know, only *one* ... system of axioms [for set theory] has found rather general acceptance, namely, that constructed by Zermelo. Russell and Whitehead, too, constructed a system of logic that provides a foundation for set theory; if I am not mistaken, however, mathematicians have taken but little interest in it. In what follows I therefore concern myself almost exclusively with Zermelo's axiomatization,...[p.291]

Indeed, as intimated above, with one notable qualification (discussed by Skolem in his fourth remark), Zermelo's system was an apt target for Skolem's criticisms in the sense that it appeared to be a sufficient foundation for Cantorian set theory and extant mathematics - at least in so far as an instrumentalistic attitude is maintained. The positive factors, then, which prompted Skolem to concentrate on a first-order system are to be located in certain features of Zermelo's 1908a.

Zermelo's axiomatization was a higher-order order system and this is tacitly acknowledged by Skolem when he transforms the axiom of separation into a schema. But by turning Zermelo's axiomatization into a first-order system how can it be that he is attacking *Zermelo's system* - the only axiomatic foundation that at the time had "found general acceptance"?

The answer is that, in a certain sense, Zermelo's 1908a, despite his stated characterization of definite property and subsequent formulation of the axiom of separation, is in effect, or *practically* speaking, a first-order theory. I mean by this the following:

i) As Skolem notes, all the axioms apart from the axiom of separation involve only quantification over sets (and perhaps urelements);

ii) Throughout the 1908a, including the development of cardinality theory, in the *application* of the axiom of separation, Zermelo never uses more than *particular* instances of the axiom, i.e. never the full second-order axiom;

iii) In each application, construing 'definite propositional functions' as syntactic objects - which is a natural construal in the context - only predicates of the first order theory of membership are employed. In other words, Zermelo never uses a definite property that isn't identifiable as falling within those characterized by Skolem.

Thus, if we confine ourselves to analysing Zermelo's constructions and proofs, Skolem's notion may be 'read off' so to speak. The only discontinuity, albeit a minor one, in this respect is that Skolem jumped to a schema including *all* instances of predicates of the first-order theory of membership. It is true that Skolem would have had to convince himself that his notion were sufficient for further purposes, i.e. the foundational sufficiency of the residual system. But, as stated above, Zermelo's system already appeared sufficient, and he employed what was essentially subsumed in Skolem's system. Incidentally, we noted Skolem's claim that his notion of definite property is "completely clear" and "sufficiently comprehensive to permit us to carry out all ordinary set-theoretic proofs." The first part of the claim refers directly to the effectiveness of the notion. (Skolem's system is a paradigm of precisification.) From the above discussion it is not unreasonable to conclude that the proofs Skolem had in mind were those in Zermelo's 1908a.

(It is appropriate to interject here a note on Skolem's axiom of replacement. The qualification with respect to the sufficiency of Zermelo's system referred to above was that the existence of the set $\{\omega, P\omega, PP\omega, \ldots\}$ was not provable in Zermelo's system and hence his theory was judged deficient in providing full Cantorian transfinite arithmetic. This was noticed independently by Skolem and Fraenkel. To remedy this deficiency they introduced an axiom of replacement. Each of them framed such an axiom in terms of their respective notion of definite property. Roughly speaking, the idea was that if a definite property behaves functionally over a set then the image is also a set. It is true that a more precise formulation of definite property in turn renders the axiom of replacement more precise and easier to work with, for instance, in connection with models of the theory. But a more workable axiom of replacement is rather a welcome spin-off from both mens refomulation of Zermelo's notion of definite property rather than a direct motivation for it. In Fraenkel's case, his reformulation stemmed from a need for a sharper version of the axiom of replacement in order to push through his demonstration of the independence of the axiom of choice. [See Fraenkel 1922])

iv) I turn now to four further topics arising from the concerns of this chapter.

(1) Skolem and reduction.

Skolem was considerably influenced by the work and concerns of Peirce, Schröder and Löwenheim. A common link between these three was the tradition, which can be traced back to Boole, of an algebraic treatment of logical operators. More specifically, a central influence on Skolem was Löwenheim's 1915. This paper, as van Heijenoort informs us

> deals with problems connected with the validity, in different domains, of formulas of the first-order predicate calculus and with various aspects of the reduction and decision problems. All these topics had remained alien to the trend that had become dominant in logic, that of Frege-Peano-Russell. In the following decades, however, these problems were to come more and more into the foreground, and the paper is now rightly considered a pioneer in logic. [p.228]

Löwenheim held the view that all of mathematics could be framed within his 'relative calculus' - a system of higher-order logic incorporating a Boolean algebra of classes and based on the system of Schröder's 1895 - as was, albeit in modified form, Skolem's logic. In his 1915, which was very well known to Skolem, Löwenheim states

> Every theorem of mathematics...can be written as a relative equation; the mathematical theorem then stands or falls according as the equation is satisfied or not. This transformation of arbitrary mathematical theorems into the relative equations can be carried out, I believe, by anyone who knows the work of Whitehead and Russell. [p.246]

This passage occurs in a section entitled "Reduction of the Higher Calculus of Relatives to the Binary." Now directly following this passage he writes

> Since, now, according to our theorem the whole relative calculus can be reduced to the binary relative calculus, it follows that we can decide whether an arbitrary mathematical proposition is true provided we can decide whether a binary relative equation is identically satisfied or not.

Now for those, like Skolem, greatly concerned with foundational issues, it would of been of considerable interest that the process of reducing mathematics could be further extended to a first-order fragment of the relative calculus.

(2) Skolem and Completeness.

It is worth noting that one argument that is standardly proposed favouring first-order logic, namely its completeness with respect to an intuitively correct notion of validity, was unavailable to Skolem. In his proof of Löwenheim's theorem Skolem had provided the essential mathematical steps for the completeness result but unlike Gödel did not have a sufficiently clear insight into the distinction between syntax and semantics to utilize what he had at hand. In particular Skolem conflated 'satisfiable' and 'consistent'. This is explained by Wang "by the fact that Skolem is in the tradition of Boole, Schröder, Löwenheim, and Korselt. Acording to this tradition, unlike that of Frege and Hilbert, logic is not thought of as a deductive system. From this point of view, as Professor Bernays has pointed out, satisfiability is the same as consistency (non-contradiction)...." [1970 p.22] Dreben and van Heijenoort have taken up this line of argument in a recent collection of Gödel's papers. They write

> For Peirce, Schröder and their followers...quantificational formulas were indeed often at the centre of their attention, but...the very notion of formal system was absent. Thus in his fundamental 1915 Löwenheim deals with quantificational formulas with identity. His approach, however, is purely model theoretic, that is, semantic. He has no formal axioms or rules of inference for quantification theory. His basic notion is that of the truth of a formula for a given interpretation in a given domain, and with that he handles validity and satisfiability. Obviously, no question of completeness of a formal system could arise here either. [1986 p.45]

Despite Skolem's failure to provide arguments for the priority of a first-order formulation of set theory and despite his highlighting of the first-order 'diseases' he was certainly influential in its establishment as the standard presentation.

The criticisms put forward in his 1922 are not sufficient for Skolem's case against the application of the axiomatic method to set theory. In particular, there are two gaps in his argument. First, he needs to explain why we should go along with his shift to a first-order system. Secondly, even granting the first, he has to convince us that the first-order diseases inherent in a first-order axiomatic set theory militates against its foundational viability. For example, Skolem does not provide *direct* arguments as to why a non-categoric theory, constituting a 'core' theory of sets, is not viable as a foundation. Indeed, since he tended towards a relativistic view of mathematical concepts, this richness in the model theory would, presumably, come to be perceived as a bonus. But as I said, Skolem did not follow through on these matters in any detail and resolve what appear as tensions in his attitude to the foundational viability of axiomatic set theory.

(3) Skolem and the semantic paradoxes.

Zermelo introduced the notion of definite property in order to avert the semantic paradoxes. Whilst Skolem's system retains a notion of definite property there is no mention in his paper of the semantic paradoxes. Presumably he did not consider them a threat. Is this because, like Peano and in contrast to Zermelo, he believed that a paradox such as Richard's "does not belong to mathematics, but to linguistics" and consequently could not emerge from within a mathematical system such as axiomatic set theory? [See Peano 1906]

The topic of semantic paradoxes is rarely addressed in connection with present day axiomatizations of set theory. This is in large measure due to the received wisdom that semantic paradoxes do not encroach into the domain of formal systems. A key influence here is Ramsey's paper of 1926, where, applying some Wittgensteinian theses, he determines to demonstrate that simple type theory is sufficient to carry, with impunity, the logicist programme. How far Ramsey's arguments are convincing with respect to formalized set theory in general is problematic. But in any case he seems to have been effective in convincing the majority of workers in mathematical logic that the semantic paradoxes do not constitute a threat for formal systems.

Skolem's paper, however, precedes Ramsey's by four years; so we may assume that the latter's arguments play no part in Skolem's reasons for thinking his system was protected from the semantic paradoxes. Rather than looking at the tack taken by Peano and Ramsey I would claim that a more plausible account of Skolem's thinking is to be located in the fact that his definite properties were, in the sense discussed above, a subclass of Zermelo's definite properties. Put bluntly, Skolem tackled the semantic paradoxes (*and indeed the logical paradoxes*) by the same means as Zermelo. That is, he judged his 'properties', for the purpose of separation, to be 'definite' in the relevant sense. Since he constrained the extension of his definite properties to the type that Zermelo was using *in practice* he did not require the added prescription that "the fundamental relations of the domain, by means of the axioms and the universally valid laws of logic, determine without arbitrariness whether it holds or not." It was evident, at least to Skolem, that this prescription was satisfied by this class of definite properties.

But Zermelo *was* worried that Skolem's general approach would invoke the semantic paradoxes. In this connection it was worth quoting the following passage from his 1932 which not only provides insight into his anti-formalism and what I interpret to be his conceptualist leanings but also illustrates his resistance to adopting the path that was to take set theory to its present standard form i.e. the system ZF.

From the assumption that all mathematical concepts and theorems must be representable by a *fixed finite system of signs*, one falls inevitably into '*Richards Paradox*'. Recently this paradox, long after it seemed dead and buried, found a happy resurrection in *Skolemism*, the doctrine that *every mathematical theory, in particular set theory, is satisfiable in a countable model*. As is well known, from contradictory premises one may deduce whatever one wishes. Even the strangest consequences which Skolem and others have drawn from their basic assumptions, e.g. the 'relativity' of the concepts of power set and equipollence, have not sufficed to turn them away from a doctrine which for many has seemed to degenerate into a dogma beyond criticism. However, a healthy 'metamathematics', a true 'logic of the infinite', first becomes possible through a *fundamental renunciation* of the assumption characterized above, which I term the *'finitistic prejudice'*. In any case the true subject matter of mathematics is *not*, as many would have it, 'combination of signs' but *conceptually-ideal relations* between the elements of conceptually determined *infinite manifold*. Thus our systems of signs is always an *incomplete* device, shifting from case to case. It reflects our *finite* understanding of the infinite, which we cannot *immediately* and *intuitively* 'survey' or comprehend, though at least we can approach mastery step by step. In the following an attempt is made to develop the foundations of a 'mathematical logic' which, free from the 'finitistic prejudice' and from inner contradictions, offers enough room for the whole of mathematics as it exists at present (and for its fruitful future development (while *abandoning* all *arbitrary prohibitions* and *restrictions*.) [Zermelo 1932 p.85. Moore's translation in his 1980]

As a footnote, it must be said, that, Zermelo was not entirely misguided (contrary to the Ramsey's view) to worry about the emergence of semantic paradoxes in formal systems. For example, consider their exploitation by Gödel:

After finishing his doctoral dissertation 1929, Gödel set to work to prove the consistency of analysis, pursuant to Hilbert's program. He proposed to divide the difficulties of the problem by first reducing the consistency of analysis to that of number theory. He began by considering the model in which the set variables are interpreted as ranging over sets definable in arithmetic. He soon realized that he would need not just the consistency of number theory but also its truth. This led him to ponder Richard's paradox and the paradox of the liar, a formal analog of which can be used to infer that truth in number theory cannot be defined in number theory (cf. *Wang* 1981, page 654). However, provability in number theory can be defined in number theory. Therefore, if the provable formulas are all true, there must be some true but unprovable formulas. Thus Gödel came to find the results he published in *1931*, which shook to its foundations Hilbert's program for foundations without quite demolishing it. [Kleene 1986 p.127]

Since semantic notions, albeit in an encoded or implicit mode, can be located within formal systems; it is premature to dismiss the spectre of the semantic paradoxes as having been exorcized.

PART III: THE GENERALIZATION OF SET THEORY.

INTRODUCTION

The fundamental question under discussion is the philosophical and foundational significance of Cohen's independence results. Parts I and II, though they may be read as self-contained essays provide the context for an appraisal of the philosophical significance of Cohen's independence results and a basis for the resolution of foundational issues brought into high relief by these results. The resolution takes the form of adopting as a foundation a generalized concept of set, namely that embodied in 'Local Set Theory' (LST). Underlying LST and in essence incorporated into it is the topos theoretic outlook and, in a sense to be specified, topos theory itself.

In his paper entitled "From Absolute to Local Mathematics" Bell proposes that we " abandon the unique absolute universe of sets central to the orthodox set-theoretic account of the foundations of mathematics, replacing it by a plurality of local mathematical frameworks- *elementary toposes*-..." [1986 p.409-] This is in accord with the underlying philosophy of topos theory itself. Johnstone explains that the topos theoretic outlook

> ...consists in the rejection of the idea that there is a fixed universe of "constant" sets within which mathematics can and should be developed, and the recognition that the notion of "variable structure" may be more conveniently handled within a universe of *continuously variable* sets than by the method, traditional since the rise of abstract set theory, of considering separately a domain of variation (i.e. a topological space) and a succession of constant structures attached to the points of this domain. In the words of F.W. Lawvere [1975], "Every notion of constancy is relative, being derived perceptually or conceptually as a limiting case of variation, and the undisputed value of such notions in clarifying variation is always limited by that origin. This applies in particular to the notion of

constant set, and explains why so much of naive set theory carries over in some form into the theory of variable sets". It is this generalization of ideas from constant to variable sets which lies at the heart of topos theory; [1977 p.xvii]

My purpose here is to establish that the generalization of set theory inherent in this move to local frameworks, more specifically in its incarnation as LST, is the appropriate progressive response to foundational challenge of post-Cohen mathematics. In addition, my purpose is to establish that the considerations of parts I and II, taken in conjunction, direct us to this generalization and that this generalization is the natural evolutionary step beyond ZF in the context of the mathematical approach.

ZF is a formalization of an informal body of mathematics and as such the two desiderata of codification and precisification are realised in the sense discussed in part I. But this informal mathematics is not fruitfully construed as being concerned with some realm of abstract objects or body of knowledge. Rather, the informal mathematics underlying ZF is the outcome of a complex *conceptual* development. The key features of this development constituted the subject matter of Part II.

ZF is a product of the mathematical approach to set theory. Understood as a *mathematical* theory and as the formalization of a *concept* justice can be done to the affinities between ZF and the formal axiomatic realizations of algebraic concepts; particularly with respect to their development and treatment. In the recognition that the enterprise of formalizing set theory (within the mathematical approach) carried with it the hallmarks of the development and formalization of an algebraic concept, involving as it does an emphasis on structure, operations and models, the nature of the shift from ZF to topos theory is readily appreciated. It is a process naturally involved in the development of algebraic concepts, namely: *generalization*. In this way the notion of set underlying ZF can be seen to be a particular instance of some general notion. In their category-theoretic characterization of the notion of set (more accurately 'model of set theory') i.e. elementary topos theory, Lawvere and Tierney provided such a generalization. This generalization assumes its optimum form as LST.

Generalization, in fact, turns out to figure highly in some key parts of our discussion, e.g. in the section on the generalization of the classical notion of 'sheaf'. However, that the notion of a topos is a generalization of the classical notion of set is, of course, *in itself* not an argument in favour of topos theory. There must also be concomitant foundational advantages. Thus in this connection we may to some extent agree with Kline, that

> Generalization and abstraction undertaken solely because research papers characterized by them can be written are usually worthless for application. In fact, most of these papers are devoted to a reformulation in more general or more abstract terms or in new terminology of what had previusly existed in more concrete and specific language. And this reformulation provides no gain in power or insight to one who would apply the mathematics. The proliferation of terminology, largely artificial and with no relation to physical ideas, is certainly not a contribution but rather a hindrance to the use of mathematics. It is a new language but not a new mathematics. [1980 p.283]

Kline overstates the case, however. Even in the most abstract realms of abstract algebra the adoption of any particular generalization of a concept is required to carry with it progressive elements; for example: new theorems, gains in simplicity, insight into existing concepts and generation of new mathematics or progressive mathematical programmes. Now, so far as the generalization of set theory in the topos framework is concerned, these elements are present in abundance. It is my conviction that this foundational approach will increasingly gain ground, not only in abstract mathematics but in applications. In fact it is already being applied to concepts in computation theory and physics. [See e.g. Manes 1975, Davis 1977, Jozsa 1979] And as for future applications Bell has expressed his optimism thus

The replacement of absolute by local mathematics results, in my view, in a considerable gain in *flexibility of application* of mathematical ideas, and so offers the possibility of providing an explanation of their "unreasonable effectiveness" (cf. Wigner 1960). For now, instead of being obliged to force an intuitively given concept into the Procrustean bed of absolute mathematics, often distorting its meaning in the process, we are at liberty to *choose* the local mathematics naturally fitted to express and develop the concept. To the extent that the given concept embodies aspects of (our experience of) the objective world, so also will the associated local mathematics; the "effectiveness" of the latter, i.e. its conformability with the objective world, therefore loses its "unreasonableness" and instead is shown to be a product of design." [1986 p.425]

My claim that the adoption of a topos-theoretic approach is the appropriate foundational response to Cohen's results is based on both 'external' and 'internal' considerations, which, though intimately related, are essentially distinct. Both are necessary to the claim. The external considerations are those drawn from parts I and II. These concern the formalization of mathematics and in particular set theory; as well as certain themes drawn from a more historically orientated account of the enterprise of axiomatizing set theory which play a crucial role in providing the proper context for appraising the independence results and a guide for the resolution of foundational difficulties raised by them. The internal considerations relate to the development and nature of topos theory itself and involve an account of the topos-theoretic approach. Now topos theory emerged from and will be seen to be a natural outcome of the overall evolution and character of twentieth century mathematics. The essential elements of this evolution are set theory, its application, formalization, the emphasis on structure in modern mathematics and category theory. First impressions notwithstanding these are all in harmony and combine to generate the foundational power of the topos theoretic approach. It is important to emphasize all of these factors and their peculiar contributions to this evolution.

As well as the sufficiency of the topos-theoretic approach as a foundation and indeed its progressiveness as such, a *central* feature is the light shed on the independence results when the forcing technique is analysed in a topos-theoretic setting. This analysis was undertaken by Lawvere and Tierney in the years 1969-70 and presented by Tierney in his 1972 paper entitled "Sheaf Theory and the Continuum Hypothesis" first presented at the Dalhousie conference on "Connections between Category Theory and Algebraic Geometry and Intuitionistic Logic". As Johnstone informs us:

One of the most striking applications of elementary topos theory was to give a categorical proof that the Continuum Hypothesis is independent of the axioms of set theory. This fact was first proved in 1963 by P.J.Cohen..;whilst categorical ideas are implicit in Cohen's work, the language of elementary topos theory was required to make the connection explicit and indeed the desire to do this was one of the main driving forces behind the development of elementary topos theory by Lawvere and Tierney...[(1977) pp.323-324]

Broadly speaking then, what is presented below involves the interconnections, often quite surprising, among four disciplines: set theory, category theory, topos theory and LST. After giving an account of the relevant aspects of the development of topos theory and the topos theoretic approach I concentrate on the following specific topics which lie at the heart of the contention that topos theory is the appropriate response to the foundational issues following in the wake of Cohen's independence results. These are: the algebraic view of set theory; LST and definite properties; the topos explication of the forcing technique; and the foundational progress inherent in the topos theoretic view. But first of all I discuss the foundational credentials of category theory, a parent of topos theory, and whose foundational features are inherited by its offspring.

(i) LST is set theory.

The proposal, *vis a vis* foundations, that we adopt the topos theoretic outlook, specifically in its guise as LST is not to suggest that we abandon set theory as a foundation. In particular it is not an indirect means of *replacing* set theoretical foundations by categorial foundations. LST *is* set theory. Thus my position is consistent with the view that although the possibility of a theory emerging that manifestly supersedes set theory as a foundation cannot be ruled out, and despite problems regarding the shortcomings of set theory in providing certain key constructions for category theory, so far as a foundation for practically all present-day mathematics is concerned, the notion set and concomitant set theoretical constructions are not *as yet* superseded. Noting the "remarkable fact that mathematics can be based on set theory" Blass comments that

> It is not clear to me whether this fact is a mathematical one, a historical one, or a psychological one (or something else). Does set theory have some essential structural property that guarantees its ability to encode other theories? Does set theory serve as a foundation for merely those theories that have been constructed in the past, with no expectation that it will serve for future theories? Or is there something about human brains that prevents them from producing mathematics that cannot be coded in set theory? My guess is that the historical view is closest to the truth, but for psychological reasons; mathematics codable into set theory was produced first (and we have not progressed beyond it) because it is easier for our minds to grasp. I also suspect that we have not yet come close to grasping the full complexity of what can be coded in set theory, so non-codable theories will probabably not arise (naturally) for quite some time.
> [A.Blass 1984 p.26]

But any consideration of these puzzles must take into account that the notion of set is dynamic and flexible. That is, included in, and interacting with, the conceptual growth and conceptual evolution of mathematics (as opposed to, say, the growth of mathematical knowledge, or more simply: the list of theorems proved) is the notion of set. This view is becoming increasingly evident in the literature, albeit sometimes implicitly. For example, after informing us that

> The Greek word 'arithmos', which is usually translated misleadingly as 'number', was used to designate a plurality of definite size: a determinate number of definite distinct objects.[Mayberry 1986 p.431]

Mayberry goes on to claim that

> ...the [Cantorian] concept of set is a straightforward generalization of the classical concept of *arithmos*. [ibid p.432]

Gray is explicit. He forthrightly states that

> On the broadest possible time scale, there have been three great advances in mathematics: Euclidean geometry, Newtonian and Leibnitzean calculus, and Cantorian set theory. Each was a culmination of decades or centuries of fragmentary results and each provided the impetus for vast new systematizations of knowledge. In each case the original insights required extensive modifications and improvements (those in geometry not appearing until the 20th century). As far as set theory is concerned, what is clear is that we are still in the middle of this advance and it is too soon to speculate as to its final form. [1984 p.1]

But it must be stressed that from the evolutionary view it does not follow that the concept of set is clear. It is quite consistent with, for example, Hallett's contention that the concept of set is

> ..not an ancient, well understood concept which can easily be taken as an axiomatic primitive in the knowledge that it can be supported by extra axiomatic explanation. (...this is largely *why* set theory is axiomatized, because we do *not* understand the set concept well) [Hallett 1984 p.300]

The essential point is that the enterprise of formalizing and axiomatizing set theory reflects the growth and development of a concept. A major factor in this development has been generalization and LST is a further generalization of the set concept. But having stated that in espousing LST one is remaining entrenched within set theoretical foundations I seem to have generated a certain tension. For I stated that LST incorporates the topos theoretic point of view and, as explained below, in a certain sense, *is* topos theory. However, topos theory is an outgrowth of category theory. In fact, formally it can be presented as a finite extension of the first order theory of categories. (Of course to *merely* comprehend it as such belies its foundational power just as we do no justice to ZF by simply taking it as merely another extension of first order logic.) Furthermore, topos theory, as such, carries along with it a significant amount of category theoretic thinking both in terms of heuristics and techniques.

Have I not then eschewed set theoretical foundations for categorial foundations? Whilst it is true that it is important for my purposes that the powerful insights and machinery of category theory are applicable to topoi, it is their role as set theoretical sites for mathematical activity, i.e. as 'models' of set theory that is crucial. Moreover, their formulation by means of LST serves to *dispel* this tension. We shall discuss these points later. At present, however, it is appropriate to discuss some relevent aspects of category theory. For the growth and development of category theory will play a crucial role in our discussion. Amongst other things, category theory has contributed important general foundational concepts and the interweaving of set-theoretical foundations and the categorial viewpoint is a key to addressing the foundational significance of Cohen's results.

(ii) Category theory and its foundational importance

(iia) the emergence of category theory and emphasis on structure in modern mathematics.

The potential foundational importance of category theory derives from the importance of mathematics as a study of structure or as structure as the focus of mathematical investigation. From Hypatia, who is reckoned to be the first woman mathematician in history, through to Emmy Noether, a student of Hilbert and who in turn tutored mathematicians ('the Noether boys') such as Artin and van der Waerden, the history of mathematics displays a continuous concern with what we may now identify as algebra. In its formative stages algebraists concerned themselves with methods for solving equations which in turn gave rise to extension of number systems by the adjunctions of, for example, 'imaginary' numbers and subsequently varieties of 'complex' numbers. However, this concern usually played a very minor role in mathematical activity overall. But in the nineteenth century this interest grew ever more marked, and moved from the study of particular systems, e.g. vectors, matrices, quarternions and a variety of hypernumbers, to more abstract notions. According to Kline

The first abstract structure to be introduced was the group. A great many of the basic ideas of abstract group theory can be found implicitly and explicitly at least as far back as 1800. [1972 p.1137]

Quite early on it began to become apparent that certain systems were a particular kind of more general system or rather 'species' of system. For example, as Kramer informs us:

...in 1844, just a year after Hamilton published the details of his quaternion algebra, the German geometer Hermann Grassmann (1809-1877) formulated a far more general concept of algebras of hypercomplex numbers of all orders...In fact, Hamilton's quarternions became just one special algebra among the many Grassmann species. [1970 p.79]

Nineteenth century mathematics is replete with such examples. Of course, the actual history of these developments is quite convoluted. However, the increasing awareness of the interconnections and analogies between the increasing number of systems and their operations led to greater degrees of abstraction as well as a need for some natural principles of unification and simplification. The development of modern axiomatics, set theory and category theory each contributed to this need. Before making some brief comments on the role of the first two, note the key general pattern and its connection with the evolution to category theoretic ideas as presented by Bell in the the following typically elegant sketch:

Category theory... provides a general apparatus for dealing with mathematical structures and their mutual relations and transformations. Invented by Eilenberg and MacLane in the 1940's, it arose as a branch of algebra by way of topology, but quickly transcended its origins. Category theory may be said to bear the same relation to abstract algebra as the latter does to elementary algebra. For elementary algebra results from the replacement of *constant quantities* (i.e. numbers) by *variables*, keeping the operations on these quantities fixed. Abstract algebra, in its turn, carries this a stage further by allowing the *operations* to vary while ensuring that the resulting mathematical structures retain a certain prescribed *form* (groups, rings, or what have you). Finally ,category allows even the form of the structures to vary, giving rise to a general theory of mathematical structure or form. Thus the genesis of category theory is an instance of the dialectical process of replacing the *constant* by the *variable*,...[1986 p.409-10]

It must be stressed, particularly in the present climate of somewhat philistine attacks on pure mathematics [see, for example, Kline's 1980], that the development and generalization of algebraic concepts was and is on no reasonable view sterile. In any case, the history of mathematics is full of examples of concepts that were not immediately perceived to be instrumental or whose widespread applicability came very much as a surprise and even continues to astonish by its power. Perhaps the most dramatic example is the notion of a group.

The idea of a Group is one of the great unifying ideas of mathematics. It arises in the study of symmetries, both of mathematical and of scientific objects. Very surprisingly, the examination of these symmetries leads to deep insights which are not available by direct inspection: while the notion of a group is very easy to explain, the applications of this concept do not at all lie on the surface. In mathematics the concept of a group is fundamental to the fields of differential geometry topology, number theory and harmonic analysis, while in science this idea is essential in spectroscopy, crystalography, and atomic and particle physics. The importance of abstraction is nowhere more evident than in the concept of a group. [J.L.Alperin 'groups and symmetry' 1980 in Mathematics Today ed. Steen]

In fact Poincaré was once inspired to uninhibitedly exclaim that "The theory of groups is *all* of mathematics." But we may also consider the initially more modest example of the 'imaginary' numbers developed to satisfy the aesthetic requirements of sixteenth century equation solvers.

...the complex numbers $z=x+iy$ have turned out to be much more than just a system of numbers. Ordinary functions such as x^2, 2^x, or $\sin x$ can be extended to make sense for complex numbers, so that one may form $w=z^2$ or $w=2^z$ or $w=\sin z$ for z and w both complex. This discovery led to a theory of functions of a complex variable, called analytic functions. This theory starts with the extension of calculus to complex functions of a complex argument z,.... The resulting theory of analytic functions of a single complex variable was the great masterwork of nineteenth century mathematics. Its impact on physics can be measured by the following sentence which opens a recent treatise on the physics of fundamental particles: "The great discovery of theoretical physics in the last decade has been the complex plane."

One of the major pure mathematical themes of the three past decades has been the extension (begun by Karl Weierstrass, Henri Poincaré, and Friedrich Hartogs at the end of the nineteenth century) of this theory to the theory of analytic of several complex variables....While the theory of analytic functions of several complex variables has pressed forward in recent decades, mathematicians and physicists in the past decade have used such sophisticated results to calculate certain integrals invented by the physicist Richard Feynman in quantum field theory. [Browder and MacLane 'The relevance of mathematics' 1980 in 'Mathematics Today' ed. Steen]

The axiomatics of the nineteenth century marks the beginning of the codification and precisification of the ever increasing number of algebraic concepts. In the context of nineteenth century algebra axiomatization is very much the starting point and catalyst of abstraction. The history of geometry bears witness that in general the axiomatization of a discipline in itself does not so swiftly bring about any significant abstraction. But within algebra with its early toleration of 'imaginary' numbers and subsequently the hypercomplex, and so on, it was reasonably clear that one was (relatively) freely operating with concepts and the inhibitions and phobias of the early workers on non-euclidean geometry were not appropriate. With the input of Hilbert and his school and the advent of set theory the important shift from axiomatization to formalization got under way and with it abstract algebra in its contemporary guise. But it is interesting to note that the axiomatic approach in algebra started surprisingly early on in the nineteenth century. (This in the period before Hamilton, who is by many considered the founder of abstract algebra by virtue of the non-commutivity of his quaternions.) This approach was developed by the British algebraic school, which included; Peacock, Farquharson, Gregory, de Morgan and Boole. [See Kramer 1970 pp.79-80]

Reid recounts the following tale:

In his docent days Hilbert had attended a lecture in Halle by Hermann Wiener on the foundations and structure of geometry. In the station in Berlin on his way back to Königsberg, under the influence of Wiener's abstract point of view in dealing with geometric entities, he had remarked thoughtfully to his companions: "One must be able to say at all times - instead of points, straight lines, and planes - tables, chairs, and beer mugs." [1972 p.57]

The story may be apocryphal. But we may find in it a position or attitude attributable to Hilbert that, in combination with the development of set theory, came to have an immense influence. This attitude is very much evident in Hilbert's *Grundlagen der Geometrie* of 1899. (We have to some extent discussed it in relation to the topic of implicit definition. It is also important to bear in mind, as we shall discuss below, that Zermelo was a student of Hilbert at that time and was heavily influenced by his general viewpoint.) Although Hilbert was to develop different and certainly elaborated metamathematical views following Brouwer's attack on classical mathematics , i.e. those associated with Hilbert's formalistic programme, this early attitude became widespread and was passed on to, amongst others, the pioneers of contemporary algebra. But it is important to note that the influence of the early Hilbert made itself felt throughout general mathematics. Within algebra this legacy is reflected, for example, in the following passage from the preface of MacLane and Birkhoff

The "modern" approach to algebra rests on the use of axioms for groups, rings, fields, lattices, and vector spaces as a means to understanding algebraic manipulations. This modern approach became generally accepted on the graduate level shortly after the publication in 1930 and 1931 of Van der Waarden's now classic *Moderne Algebra* [Preface to the second edition 1979 of 'Algebra' 1967]

But left unstated in this passage is the input of set theory for the provision of domains and constructions on them. That is, in varying degrees of formality, model theory. (However, note that, as in most modern texts, they acknowledge set theory in the first chapter.)

Put bluntly, the attitude in question is that so far as interpretation of a given axiomatization is concerned the particular domain of interpretation or the 'points of the model' is irrelevant. The centre of attention rests upon structural considerations and in particular there is no relevant mathematical distinction to be made between models bearing identical structure. But at the same time the notion of a universe of sets and universal set theoretical constructions became an ever more important adjunction to this attitude. [A good idea of the landmarks of the model theory may be gained from Vaught 1974 'Model Theory before 1945' and Chang 1974 'Model Theory 1945-1971']

The input of set theory, then, is to provide structures and constructions and operations on structures. Notions such as ***homomorphism*** and ***isomorphism*** central to structural concerns are able to take on a significant degree of rigour. Furthermore together with the idea of a universe of sets emerges a measure of coherence to the locutions '*all* groups', '*all* fields', and so on. That is, reference to 'groups' or propositions about them may be construed as reference or propositions about a particular class of sets; roughly speaking, the 'category' of groups. This move also facilitates the investigation of global questions: the sort of question generally answered by model theory rather than the generation of theorems from a collection of axioms. This is an important feature for mathematical practice - as Hodges illustrates in the following passage:

It is often said that in an 'axiomatic theory' such as group theory, the axioms are 'assumed' and the remaining results are 'deduced from the axioms'. This is completely wrong. W.R. Scott's textbook *Group Theory* [1964] contains 457 pages of facts about groups, and the last fact which can by any stretch of the imagination be described as being 'deduced from..[the axioms]' occurs on page 8. [1983 p.72]

Now the global picture of mathematics we have arrived at is of an increasingly proliferating variety and complex investigation of mathematical concepts realized as structures as well as the investigation of their relationships. All this is referred to a universe of sets for which the 'categories' are not visible; they are not themselves mathematical objects in the given arena of mathematical discourse. Now as it happens, as we shall presently see, the category theory of MacLane and Eilenberg was not a *direct* response to the need for principles lending coherence, unification and simplification to the situation at hand. However

Like set theory, it provides a general framework for dealing with mathematical structures, and - again like set theory - it achieves this by transcending the *particularity* of structures. But set theory and category theory go about doing this in entirely different ways. Set theory strips away structure from the ontology of mathematics leaving pluralities of structureless individuals open to the imposition of new structure. Category theory, on the other hand, transcends particular structure, not by doing away with it, but by *generalising* it, that is, by producing an *axiomatic general theory of structure*. The success of category theory, and its significance for foundations is due to the *ubiquity of structure* in mathematics.
[Bell 1981 p.349]

(iib) The Development of Category Theory.

In the introduction to their seminal paper 'General Theory of Natural Equivalences' (1945) Eilenberg and MacLane state that

> In a metamathematical sense our theory provides general concepts applicable to all branches of abstract mathematics, and so contributes to the current trend toward uniform treatment of different mathematical disciplines. In particular, it provides opportunities for the comparison of constructions and of the isomorphisms occuring in different branches of mathematics; in this way it may occasionally suggest new results by analogy.
>
> The theory also emphasizes that, whenever new abstract objects are constructed in a specified way out of given ones, it is advisable to regard the construction of the corresponding induced mappings on these new objects as an integral part of their definition. The pursuit of this program entails a simultaneous consideration of objects and their mappings (in our terminology, this means the consideration not of individual objects but of categories). This emphasis on the specification of the type of mappings employed gives more insight into the degree of invariance of the various concepts involved. [p.236]

The class of all groups and the homomorphisms between them is a paradigm of what we may intuitively take to be a 'category'. (I say 'intuitively' because for the moment I want to ignore any foundational arrangements that have to be made in order to clarify locutions such as 'the class of all groups', e.g. fixing a universe of sets or sequence of such.) In general the picture of a category is a species of structure together with structure preserving functions between them. Now belying what turns out to be immense conceptual potency the basic details of axiomatic category theory are surprisingly straightforward. A category C is a two-sorted system consisting of what are usually referred to as 'Objects' and 'Arrows'. (I shall use X,Y,Z,..to designate objects and f,g,h,..for arrows.) Each arrow f is designated a pair of objects 'Domain' and 'Codomain' respectively and designated dom(f) and cod(f). (In category theory it is helpful to construe an arrow as an ordered triple $\langle f,X,Y\rangle$, this will usually be denoted '$f:X\to Y$'.) A pair of arrows f,g such that cod(f)=dom(g) are called 'composable' and to each such pair there corresponds a 'composition' $\langle g\cdot f,X,Z\rangle$ where X=dom(f) and Z=cod(g). Composition obeys the associative law, ie $h\cdot(g\cdot f)=(h\cdot g)\cdot f$. Finally, to each object X of C is assigned an 'identity' arrow $\langle 1_X,X,X\rangle$ which behaves as the unit of the composition operation, ie. $1_X\cdot f=f$ and $g\cdot 1_X=g$. The notion of a category given above is readily formalizable within a first order language. [See Hatcher 1982.]

Together with the category of groups, algebraic species such as: rings, modules, topological spaces and their respective morphisms are categories that readily spring to mind. But the notion allows without further ado examples such as the 'opposite' categories. Given a category C we can construct C^{op} which has the same objects as C and the same arrows except that their domains and codomains are interchanged, i.e. if $\langle f,X,Y\rangle$ is a C-arrow $\langle f,Y,X\rangle$ is a C^{op}-arrow. The important point to notice here is that the notion of an arrow is quite general and need not, unlike the group homomorphisms etc., correspond to the intuitive or set theoretical notion of a function. Opposite categories in fact will turn out to be important for the construction of a fundamental class of topoi. In this connection we might mention the category constructed from a partially ordered set P. The objects are the members of P and the arrows are induced from the ordering relation in the sense that between $p,q\in P$ there exists the single arrow $\langle p,q\rangle$ (i.e. $\langle\langle p,q\rangle,p,q\rangle$) iff $p\leq q$.

As well as being interested in certain individual categories we shall also be interested in investigating relationships between categories. For example, groups, rings, modules, fields and vector spaces, have important structural relationships. The fundamental tool for this investigation is the 'Functor'. This may be construed as a morphism on categories or a structure preserving function on categories. I shall denote a functor between categories C, D by $F:C\to D$. F sends each C-object X to a D-object, $F(X)$; and each C-arrow $f:X\to Y$ to a D-arrow $F(f):F(X)\to F(Y)$ in such a way that composition is preserved, ie. $F(g\cdot f)=F(g)\cdot F(f)$. Also $F(1_X)=1_{F(X)}$. Clearly, where F is an identity function on a category, F is a functor (the identity functor). A frequently cited class of functors are the 'forgetful' functors, which as their name suggests 'forgets' or 'jettisons' some of the structure of its domain. An example of such of a functor is that from the category of rings to groups which takes a given ring, (which is a group plus some extra structure) to its underlying group and is injective on arrows.

Another important class of functors are the 'hom-functors'. Let S be the category of sets. The objects in this category are sets and the arrows are functions between them. For a category C, $C(X,Y)$ denotes the set of C-arrows from X to Y, and we assume it to be a S-object. Now for a fixed C-object Z the 'covariant hom-functor' $C(Z,-)$ sends a C-object X to $C(Z,X)$; and a C-arrow $f:X\to Y$ to the function

$$C(Z,f):C(Z,X)\to C(Z,Y)$$

which sends an arrow $g: Z \to X$ to $f \circ g: Z \to Y$.

The power of category theory rests on the fact that significant properties of species of structures and their relationships can be explicated 'externally'. That is, rather than referring to the particular members of a given structure we may instead talk in terms of the morphisms between it and other structures in the category. (This begins to inform us as to the particular importance of the hom-functor.) But more importantly, as will be amply exemplified, this external viewpoint facilitates increasing levels of abstraction and concomitantly deeper insights into the nature of mathematical concepts and activity. First let us look at a straightforward application of the external method which also is an example of how typical set-theoretic constructions are characterized (and generalized) in terms of arrows.

Let X,Y be C-objects. A 'product' of X and Y is a C-object XxY together with C-arrows $\pi_1: XxY \to X$, $\pi_2: XxY \to Y$ such that for every C-object Z and each pair of arrows $f: Z \to X$, $g: Z \to Y$ there is a *unique* arrow $\langle f,g \rangle: Z \to XxY$ such that the following diagram commutes

$$\begin{tikzcd} & Z \arrow[dl, "f"'] \arrow[d, dashed, "\langle f,g \rangle"] \arrow[dr, "g"] & \\ X & X \times Y \arrow[l, "\pi_1"] \arrow[r, "\pi_2"'] & Y \end{tikzcd}$$

[diagram 1]

This characterization specifies XxY "uniquely up to isomorphism". This feature of characterizations is typical and simply reflects category theory's concern with structural features within and between categories. In the case of S a product is the usual Cartesian product, where π_1 and π_2 are the obvious projection functions. But the category theoretic framework allows the notion of a product to take on extra dimensions of generality as well as allowing us to make an overall study of all categories where for any two arbitrary objects a product exists. But this is nowhere near the end of the story. To begin with, a product, together with a host of other familiar constructions, turns out to be a special case of a more general notion, namely a 'limit'. [See chapter 2.iv.b] A general study of categories 'closed' under certain limits has proved increasingly important in mathematics and logic e.g Cartesian closed categories. [See MacLane 1971. Lambek and Scott 1986] And then a limit turns out to be a special case of an 'universal construction'. This notion is intimately related to what is considered the most powerful and important notion to be thus far developed within the category theoretic framework, namely the 'Adjoint'. But here I am anticipating. For the moment let us note, as Bell puts it

> The generality of category theory has enabled it to play an increasingly important role in the *foundations of mathematics*. Its emergence has had the effect of subtly undermining the prevailing doctrine that all mathematical concepts are to be referred to a fixed absolute universe of sets. Category theory, in contrast, suggests that mathematical concepts and assertions should be regarded as possessing meaning only *in relation to* a variety of more or less *local* frameworks. ['From Absolute to Local Mathematics' 1986 p.410]

This is consonant with the relativistic reaction to Cohen's independence results. Bell continues:

To indicate what I mean, let us follow MacLane [1971] in considering the category-theoretic interpretation of the concept "group". From the set-theoretical point of view, the term "group" signifies a *set* (equipped with a couple of operations) satisfying certain elementary axioms expressed in terms of the elements of the set. Thus the set-theoretical interpretation of this concept is always referred to the same framework, the *universe of sets*. Now consider the category-theoretic account of the group concept. Here the reference to the "elements" of the group has been replaced by an arrows only formulation, thereby enabling the concept to be interpretable not merely in the universe of (category) of sets, but in essentially *any* category. The possibility of varying the framework of interpretation offered by category theory confers on the group concept a truly protean generality..it did not formerly appear to possess. Indeed the interpretation of the term "group" within the category of topological spaces is *topological group* within the category of differentiable manifolds it is *Lie group* and within the category of sheaves over a topologial space it is a *sheaf of groups*. [ibid pp.410-11]

In the above I have given some account of the notions 'category' and 'functor'. However, as Eilenberg and MacLane make abundantly clear and as is suggested in the title of their pionering work, the essential mathematical relationship they were concerned with was that of a 'natural equivalence' and then more generally a 'natural transformation'. Categories were required merely to provide domains and ranges for functors and functors were needed for the definition of a natural transformation. It is with the emergence of this latter notion that we may begin to argue for the the claim that category theory yields some significant foundational insight. Subsequently, with the isolation of the adjoint the claim is irresistible. To explicate the notion of natural equivalence I shall follow Eilenberg and MacLane's advice that "The subject matter of this paper is best explained by an example, such as that of the relation between a vector space L and its 'dual' or 'conjugate' space $T(L)$." [1945 pp.231-2] In fact I shall use this example and follow their account of it.

Let L be a finite dimensional real vector space. Its dual $T(L)$ is the vector space of all real valued linear functions t on L. $T(L)$ and L are isomorphic. But to demonstrate this isomorphism one must relativize to a definite set of basis vectors for L and the isomorphism is dependent on the basis chosen in the sense that for a different basis a different isomorphism will result. Now it is clear that L and its double dual $TT(L)$ are also isomorphic. However, this isomorphism $(\sigma(L))$ can be demonstrated independently of any choice of a basis. Furthermore, this isomorphism is "natural" in that in effect, simultaneously, for *all* finite dimensional vector spaces L, $L \cong TT(L)$ is exhibited. This situation may be further analysed.

Eilenberg and Maclane observe that

A discussion of the 'simultaneous' or 'natural' character of the isomorphism $L \cong TT(L)$ clearly involves a simultaneous consideration of all spaces L and all transformations λ connecting them; this entails a simultaneous consideration of the conjugate spaces $T(L)$ and the induced transformations $T(\lambda)$ connecting them;.." [1945 p.233]

Now an induced transformation from one dual space into another, is derived by the following means. Let L_1 and L_2 be finite dimensional vector spaces and λ_1 a linear transformation between them, i.e. $\lambda_1 : L_1 \to L_2$. Let t_2 be an element of $T(L_2)$. Then we have $t_2 \cdot \lambda_1 : L_1 \to R$, which is an element of $T(L_1)$. Thus we have a mapping $T(\lambda_1) : T(L_2) \to T(L_1)$ defined by setting $[T(\lambda_1)](t_2) = t_2 \cdot \lambda_1$. It can also be shown that identities are preserved and also that $T(\lambda_2 \cdot \lambda_1) = T(\lambda_1) \cdot T(\lambda_2)$. Furthermore, we can iterate this process to double dual spaces. What we have discussed above may be expressed in terms of a functor T^2 whose domain and codomain is the category of finite vector spaces.

This functor is usually called the iterated dual or conjugate functor and, in short, for given L and arrow λ, $T^2(L)=TT(L)$ and $T^2(\lambda)=TT(\lambda)$.

The relationship between the family of isomorphisms σ, T^2, the identity functor I, is that for any given arrow $\lambda: L_1 \to L_2$ the following diagram commutes

$$\begin{tikzcd}
I(L_1) \arrow[r, "\sigma(L_1)"] \arrow[d] & T^2(L_1) \arrow[d] \\
I(L_2) \arrow[r, "\sigma(L_2)"] & T^2(L_2)
\end{tikzcd}$$

[diagram 2]

It is this commutitivity, expressed equationally as $\sigma(L_2)\cdot I(\lambda)=T^2(\lambda)\cdot\sigma(L_1)$, for arbitrary λ, which yields the meaning of the 'naturality' of σ. The functors I and T^2 are said to be 'naturally equivalent.' In general then, functors $F,G:C\to D$ are naturally equivalent if their is a family of D-isomorphisms σ such that for an arbitrary C-arrow $f:X\to Y$ the equation $\sigma_Y\cdot F(f)=G(f)\cdot\sigma_X$ holds. The notion of natural equivalence may be extended by weakening the condition on σ, i.e. by allowing the components of σ to be homomorphisms. In that case we say that σ is a natural transformation of F into G. If we think of F(C) as being the 'picture' of C in D under F, then σ naturally transforms F(C) into G(C).

Eilenberg and MacLane originally formulated these notions to solve problems in homology and cohomology. [see Eilenberg and MacLane 1942.] It was early on recognized that "Some useful maps arising in geometry are clearly "natural" ones, like the map of vector space L to its double dual $T^2(L)$..[and].... Initially, it was easy enough to leave this notion informal, with the side observation that for finite-dimensional vector spaces V the usual isomorphism $V \cong V^*$ was *not* natural, because it depended on choices (here, on the choice of bases)." [MacLane 1976 p.33]. But it became clear to them that 'naturality' was a key notion and that an analysis and rigorous account was required. Furthermore they were in no doubt doubt as to the generality of the basic categorial ideas they developed. They write

The natural isomorphism $L \to T_2(L)$ [(3)] is but one example of many natural equivalences occurring in mathematics. For instance, the isomorphism of a locally compact abelian group with its twice iterated character group, most of the general isomorphisms in group theory and in the homology theory of complexes and spaces, as well as many equivalences in set theory [and] in general topology satisfy a naturality condition resembling (3). [1945 p.235]

*

In characterizing products in a category it was mentioned that characterization in category theory typically specified objects only up to isomorphism. From the category theoretic point of view two isomorphic objects are identical and isomorphic copies of an object are redundant. Thus all relevant information about a category is contained in what is referred to as its skeleton which is, roughly speaking, the category formed from a given category by removing all members but one of any isomorphism type. Using natural equivalences the category theoretic outlook just now expressed can be made rigorous. Two categories C, D are said to be 'equivalent', written $C \simeq D$, if there exist functors $F: C \to D$, $G: D \to C$ such that $G \circ F$ and $F \circ G$ are naturally equivalent to the identity functors 1_C and 1_D respectively. Thus the category of finite ordinals and the category of finite sets, though vastly different so far as the set theoretic obsession of size is concerned, are equivalent as categories.

[Note that there are still some difficulties of the kind addressed by Freyd in his 1976 paper 'Properties Invariant within Equivalence Types of Categories'. There he states

All of us know that any "mathematically relevant" property on categories is invariant within equivalence types of categories. Furthermore, we all know that any "mathematically relevant" property on objects and maps is preserved and reflected by equivalence functors. An obvious problem arises: How can we conveniently characterise such proprties? The problem is complicated by the fact that the second mentioned piece of common knowledge, that equivalence functors preserve and reflect relevant properties on objects and maps, is just plain wrong. [p.55 NB. A functor F *preserves* a property P of an arrow f if F(f) has P whenever f has. It *reflects* P if f has the property whenever F(f) has.]

The work of Eilenberg and MacLane was of far-reaching influence, as is emphatically exemplified in the work of Grothendieck - a central figure in the development of topos theory. Another important example of this influence occurs in the work of Steenrod who would claim that

no paper had influenced his thinking more than "The general theory of natural equivalences". He explained that although he had been searching for an axiomatic treatment of homology for years and that he of course knew that homology acted on maps...it had never occurred to him to try to base his axiomatics on this fact. [Barr/Wells 1985 p.62]

Although Eilenberg and MacLane published their ideas in the early and mid-forties, in MacLane's words: "These first papers on categories had no immediate sequels, because for this period they provided just a language," [1981 p.24] Steenrod's work is perhaps a notable exception. But Grothendieck's 1957 paper 'On some remarks on homological algebra' was to usher in a new era. "This paper not only summarized ideas of the previous period by presenting a definitive and 'most general' formulation of homological algebra but it also initiated the categorical emphasis of the new period." [MacLane 1981 p.25] In this paper Grothendieck axiomatized the notion of an Abelian category and went on to prove some important results. ("Thus Grothendieck demonstrated that categories could be a tool for actually *doing* mathematics and from then on the development was rapid." [Barr and Wells 1985 p.62])

Grothendieck's interest in homological algebra was as a source for methods and constructions with which to advance the theory of algebraic geometry. In this connection, his 1957 paper displayed an extremely important particular case of a categorical construction essentially requiring the notion of a natural transformation. Grothendieck's construction was that of a category of sheaves over a fixed topological space and we shall have much to say about this category in the discussion on the development of topoi. It was, however, an example of a 'Functor Category'

Having isolated the notion of a functor between two categories Eilenberg and MacLane considered categories whose objects are the functors $F:C\to D$ between two given categories C, D. These are denoted C^D where D is referred to as the 'base' category. Their motivation, which with hindsight appears staggeringly prosaic, was that a functor category "..is useful chiefly in simplifying the statements and proofs of various facts about functors,.."[1945 p.250] To construct such categories the appropriate notion of an arrow between functors was required. But this was already to hand. An arrow in a functor category is a natural transformation.

A particularly important class of these functor categories, especially so in connection with the emergence of the concept of a topos, are the categories of sheaves over a topological space. These are discussed in Chapter 5.ii.a, but the following serves as a first glance and as an appropriate example of functor categories. As motivation, consider the following construction. Let $\mathcal{X}$ be a topological space with topology $O(\mathcal{X})$. Define a set-valued mapping F on $O(\mathcal{X})$ by $F(U)=\{f: f$ a continuous real-valued function on $U\}$. For $U,V\in O(\mathcal{X})$ such that $V\leq U$ define a map $F_{UV}:F(U)\to F(V)$ by $F_{UV}(f)=f\upharpoonright V$ (f restricted to V). In categorial terms we have constructed a functor from O(X) (the category of open sets of $\mathcal{X}$ with arrows corresponding to reverse inclusions) with codomain S. The category of all such functors $G:O(\mathcal{X})\to S$, i.e. $S^{O(\mathcal{X})}$, is the category of presheaves over $\mathcal{X}$.

The particular example of a presheaf given as motivation is in fact the blueprint for the notion of sheaf. This arises from the following feature. Let $U\in O(\mathcal{X})$ and $\{U_i:i\in I\}$ a covering of U indexed by an arbitrary set I. Furthermore let $\{\sigma_i:i\in I\}$ be such that $\sigma_i\in F(U_i)$. Let $U_k=U_m\Delta U_n$ $(k,m,n\in I)$, then clearly $F_{U_m,U_k}(\sigma_m)=F_{U_n,U_k}(\sigma_n)$. In other words the functions σ_m and σ_n agree on the common part of their domains. And moreover these I-indexed functions can be 'glued' together in a unique way to form a real-valued continuous function with domain U. All this gives rise to the following definitions:

(*) A 'G-compatible family' for a presheaf G relative to an open cover $\{U_i : i \in I\}$ of U is a family $\{\sigma_i : i \in I\}$ such that $\sigma_i \in U_i$ and $G_{U_m, U_k}(\sigma_m) = G_{U_n, U_k}(\sigma_n)$ for all $k, m, n \in I$ where $U_k = U_m \Delta U_n$.

(*) A presheaf G is a sheaf if for any G-compatible family there is a unique $\sigma \in G(U)$ such that $G_{U, U_i}(\sigma) = \sigma_i$ for all $i \in I$.

The categories of the form Shv(X) of all sheaves over X in fact were the original examples of topoi.

(iic) Adjoints.

The discovery and explicit formulation of adjoints is due to Kan and was introduced in his 1958 paper. The use of the term 'adjoint' derives originally in work on linear differential operators. It was employed in connection with Hilbert spaces around 1930 where, given a Hilbert space H and a linear transformation T over H, the adjoint of T, T^*, is the linear operator such that for $x, y \in H$ inner products satisfy the condition $\langle T^*(x), y\rangle = \langle x, T(y)\rangle$. We shall see that the form of this equality is reflected in the categorial account.

There is practically universal agreement that the notion of an adjoint is the most important general idea generated from the category theoretic framework. Witness the following representative quotations

The isolation and explication of the notion of *adjointness* is perhaps the most profound contribution that category theory has made to the history of general mathematical ideas...[Goldblatt 1979 p.438]

...adjoints occur almost everywhere in many branches of Mathematics..a systematic use of all these adjunctions illuminates and clarifies these subjects [MacLane 1971 p.103]

Adjoints are everywhere [Barr and Wells 1985 p.51]

Not surprisingly, given a concept of such importance, we can find work that can be described as 'near misses' to its 'discovery'. Bourbaki, for instance, was hampered by its initial avoidance of the categorial framework provided by Eilenberg and MacLane and in a certain sense by their concentration on lines of enquiry generated within the French Mathematical tradition produced what turned out to be a *too* general notion. (I say 'in a certain sense' because this tradition was and is in many respects extremely fertile and unconstrained.) The lesson here is twofold. First, the general categorial framework is not to be underestimated or, as has sometimes been the case considered 'general abstract nonsense'. Secondly, a "a good general theory does not search for the maximum generality, but for the right generality." [MacLane 1971]

As in the case of a natural transformation, an adjoint is a relation between functors. I can only hope to give here a bare introduction to this notion. Anyway, from the quotations given above it is clear that functors so related are ubiquitous in mainstream mathematics. A particularly rich source of so related functors arises in connection with the class of forgetful functors. Lawvere, who along with Freyd, was among the first proponents of the notion, also recognised its importance with respect to discovering and analysing underlying unities within logic and foundational subjects in general. [See for example Lawvere 1963, 1964, 1969a, 1969b ,1970.] We shall see presently how they may be used to give a categorial account of quantification. As MacLane observes "The multiplicity of working examples of adjoint functors is matched by the protean forms of their definitions.." [1971a p.233] One method of giving an account of the notion, following Lambek and Scott 1986, is by treating it as a generalization of a Galois correspondence.

Recall that a partially ordered set can be regarded as a category. Likewise we can so treat a preordered set. A functor between preordered sets is an order preserving map. Let A and B be preordered sets and F a functor $F:A\to B$, thus for $a,a'\in A$, if $a\leq a'$ then $F(a)\leq F(a')$. A functor $G:B\to A$ is said to be *right adjoint* to F (and F *left adjoint* to G) if given arbitrary $a\in A$ and $b\in B$ then $F(a)\leq b$ iff $a\leq G(b)$. Notice that in such categories the hom-sets are either empty or at most contain one arrow. Trivially then, there is an induced isomorphism between each pair of hom-sets of the form $B(F(a),b)$ and $A(a,G(b))$. Furthermore, if $B(F(-),-)$ and $A(-,G(-))$ are treated as set-valued functors then the isomorphisms, again trivially in this example, constitute the components of a natural transformation between them. The above has the merit of constituting a simple example of an adjoint situation and Lambek and Scott do indeed generalize from the general idea of a Galois correspondence. The following more complicated example yields further insight into the induced natural transformation.

Let V_K be the category of all vector spaces over a fixed field K. The arrows in this category are linear transformations. For a given set X we can construct the V_K-object $V(X)$ which is the space of all K-linear combinations of elements of X, i.e. X is the set of basis vectors for $V(X)$. This yields a functor $V:S\to V_K$ taking X to $V(X)$ and for function $h:X\to Y$, the linear transformation $V(h)$ takes a vector of $V(X)$, which can be represented by a linear combination of members of X, i.e. $\Sigma k_i x_i$, to $\Sigma k_i h(x_i)$. Let $U:V_K\to S$ be the forgetful functor, i.e. $U(W)$ is the set of vectors in the space W and let $j:X\to U(V(X))$ be defined by

$j(x)=x$. Now for each function $f:X\to U(W)$ there is a *unique* extension to a linear transformation $f^*:V(X)\to W$ such that $U(f^*)\cdot j=f$. Using this fact we can define an injection from $S(X,U(W))$ into $V_K(V(X),W)$ taking f to f^*. Now let $g:V(X)\to W$ be a linear transformation. The restriction of g to X, i.e. $g\upharpoonright X$ is a function $f:X\to U(W)$ and it is not difficult to show that $f^*=g$. In other words this restriction operation is a bijection from $V_K(V(X),W)$ to $S(X,U(W))$ whose inverse is the aforementioned injection.

The above constructions have involved an arbitrary set X and K-vector space W (or as MacLane puts it "This bijection..is defined 'in the same way' for all sets and all vector spaces W." [1971 p.77]) and from the discussion of Eilenberg and MacLane's 1945 paper it is to be suspected that we have the components of a natural equivalence $\phi:V_K(V(X),W)\cong S(X,U(W))$. This is in fact the case. We may now proceed to the general characterization of an adjoint situation.

An adjoint situation is a triple $\langle\phi,F,G\rangle$. Here the functor $F:C\to D$ is said to be the left adjoint of the functor $G:D\to C$ (the right adjoint of F). ϕ is a family of maps $\phi_{a,b}$, one for each pair $\langle a,b\rangle$, $a\in C$ and $b\in D$ such that $\phi_{a,b}:D(F(a),b)\cong C(a,G(b))$ is 'natural in a and b'. We can make this naturality explicit as follows:

First, in explicating 'natural in a' b is fixed throughout. We then construct the functor $D(F(-),b):C^{op}\to S$. The functor takes an arbitrary C^{op}-object a to the hom-set $D(F(a),b)$. Now consider the C^{op}-arrow $f^{op}:a'\to a$. Then the functor requires an arrow

$D(f^{op}):D(F(a'),b)\to D(F(a),b)$. This is given by letting $D(f^{op})[g]=g\cdot F(f)$, for $g:F(a')\to b$, where $f:a\to a'$ is the C-arrow inducing f^{op}. Next we construct the functor $C(-,G(b)):C^{op}\to S$. Here a C^{op}-object is mapped to $C(a,G(b))$. An arrow $f^{op}:a'\to a$ is mapped to the function $C(f^{op}):C(a',G(b)\to C(a,G(b))$ by $C(f^{op})[h]=h\cdot f$, for $h:a'\to G(b)$ and f as above. Now natural in a amounts to the family of functions $\phi_{a,b}$ constituting a natural equivalence between the above defined functors. This works out in equational terms as, for arbitrary $f^{op}:a'\to a$, $\phi_{a,b}(g\cdot F(f))=\phi_{a',b}(g)\cdot f$. Furthermore the above holds for each b.

We now fix a. The functor $D(F(a),-):D\to S$ sends an arbitrary D-object b to the set $D(F(a),b)$. A D-arrow $g:b\to b'$ is mapped to $D(g):D(F(a),b)\to D(F(a),b')$ where $D(g)[h]=g\cdot h$. The functor $C(a,G(-):D\to S$ sends b to the set $C(a,G(b))$. A D-arrow $g:b\to b'$ is mapped to $C(a,G(b))\to C(a,G(b'))$ where $C(f)[h]=G(g)\cdot h$. Natural in b amounts to the family $\phi_{a,b}$ constituting a natural equivalence between the above defined functors. Thus the equation $\phi_{a,b'}(g\cdot h)=G(g)\cdot\phi_{a,b}(h)$. Before finishing this account of the notion of adjointness we give an example of its use in categorial logic.

The example I shall give demonstrates how quantification may be treated in a categorial context by the employment of adjoints. This treatment originated with Lawvere's program of formulating algebraic logic using categories. Here algebraic theories were certain kinds of categories. [See Lawvere 1963, 1965a, 1965b, 1969, 1970] The first step was his observation that substitution corresponds to composition of arrows. Following from this, substitution may be construed as a functor. Then in his 1965b he introduced existential and universal quantification (strictly speaking 'quantification along an arrow f') as left and right adjoints of the substitution functor.

Let $f:A\to B$ be an arrow in S. The power sets PA and PB understood as partially ordered sets are categories. The map $f^*:PB\to PA$ defined by $f^*(Y)=\{x:f(x)\in Y\}$ for all $Y\in PB$ is then easily seen to be a functor. We define the existential quantification functor along f, i.e. $E_f:PA\to PB$ as follows: $E_f(X)=\{y:\text{exists } x\in X. f(x)=y\}$. Thus $E_f(X)\leq Y$ iff $X\leq f^*(Y)$ and clearly E_f is a left adjoint to f^* i.e. satisfies the naturality conditions. We define universal quantification along f by a functor $\Pi_f:PA\to PB$ such that $\Pi_f(Y)=\{y: \text{for all } x\in A(f(x)=y\Rightarrow x\in X)\}$. Thus $f^*(Y)\leq X$ iff $Y\leq\Pi_f(X)$ and Π_f is a right adjoint to f^*.

Finally three observations. First for a given functor its left and right adjoints are unique so far as category theory is concerned. In other words any two left or right adjoints are naturally equivalent (which is isomorphism for functors.) Second, recall that general constructions within a category (I gave the example of products and it is heuristically useful to think of general set theoretic constructions) are all explicable as limits. Usually we are interested in a certain class of categories because they display a given range of limits e.g. Cartesian closed categories for the study of the λ-calculus. It turns out that adjoints are associated with important limit preservation properties. For example a functor with a left adjoint preserves limits. Furthermore we can usefully define a class of categories in terms of adjoints. Although we shall not employ this device, this is the case for topoi. [See Lawvere 1972.] Thirdly, we shall see that the forcing method itself is intimately connected with an adjoint situation.

iii) The Emergence of Topos Theory

The elementary theory of toposes was formulated by Lawvere and Tierney working together at Dalhousie university in the year 1969-70 and published in Lawvere's 1970 and Tierney's 1972. It is worth noting that both Lawvere and Tierney were former students of Eilenberg the co-founder of category theory. Thus they were familiar with the concepts, methods and theorems of algebraic topology - a discipline that would figure highly in the development of topos theory. Lawvere described their motivation as

> ...the development on the basis of elementary (first-order) axioms of "toposes" just good enough to be applicable not only to sheaf theory, algebraic geometry, global spectrum, etc. as originally envisaged by Grothendieck, Giraud, Verdier, and Hakim but also Kripke semantics, abstract proof theory, and the Cohen-Scott method for obtaining independence results in set theory... [1972 p.1]

In that this programme was fully realized it reflects the general and powerful notion of 'set' inherent in their theory. The list given by Lawvere also reflects the contributing disciplines and their techniques incorporated into and underlying elementary topos theory. In his 1976 Lawvere offered the following additional comments on the genesis of this theory

> Around 1963 (the same year in which I completed my doctoral dissertation under Professsor Eilenberg's direction) five distinct developments in geometry and logic became known, the subsequent unification of which has, I believe, forced upon us the serious consideration of a new concept of set. These were the following:
>
> "Non-Standard Analysis" (A.Robinson)
> "Independence Proofs in Set Theory (P.J.Cohen)
> "Semantics for Intuitionistic Predicate Calculus" (S.Kripke)
> "Elementary Axioms for the Category of Abstract Sets" (F.W.Lawvere)
> "The General Theory of Topoi" (J.Giraud)
> [p.102]

Lawvere thought of this concept as that of a 'variable set'. [See chapter 5.ii.c] More specifically, as Johnstone puts it

> Topos theory has its origins in two seperate lines of mathematical development, which remained distinct for nearly ten years. In order to have a balanced appreciation of the subject, I believe it is necessary to consider the history of these two lines, and to understand why they came together when they did. [1977 p.xi]

The two lines of development referred to here by Johnstone are sheaf theory which grew out of algebraic topology and Lawvere's categorial analysis of set theory. But Goldblatt is correct in pointing out that

> a full historical perspective requires the teasing out of a third strand of events... i.e. logic, especially model theory. We may begin this account with Cohen's work in 1963 on the independence of the continuum hypothesis et. al. His forcing technique proved to be the key to the universe of classical set theory, and led to a wave of exploration of that territory. But as soon as the method had been reformulated in the Scott-Solovay theory of Boolean-valued models (1965), the possibility presented itself of replacing "Boolean" by "Heyting" and thereby generalising the enterprise. Indeed Scott made this point in his 1967 lecture-notes and then took it up in his papers (1968, 1970) on the topological interpretation of intuitionistic analysis. [1979 p.xi]

The interweaving of this third strand we shall investigate in Chapter 5. Also the input of sheaf theory, both conceptually and in terms of techniques, and its interrelationships with the other two strands, for example, in connection with logic and forcing and the notion of 'variable set', is crucial and will be addressed below after some more of the basic material is presented. In this section I shall emphasize the broader background details of the emergence of topos theory and then give an account of the theory pivoting on its role as a generalized set theory.

iii.a) Grothendieck's work

The account of the development of category theory emerging from the general evolution of structurally orientated mathematics was taken as far as the formulation by Kan in 1958 of an adjoint functor. In the meantime abstract algebra had developed to the point where the categorical approach had become standard. Thus, for example, group theorists would focus on the study of 'all' groups and 'all' group-homomorphisms. (These global considerations bring group theorists directly in contact with the effects of the underlying universe of mathematical activity.) In particular "This flair for an 'overall look' appears in algebraic geometry, in model theory, and in category theory itself, as the subjects developed in this period." [MacLane 1981 p.24] Grothendieck, working in the field of algebraic geometry, was perhaps the most influential figure both in the adoption of general categorial methods and in developing, generalizing and exploiting the notion of a sheaf. In 1952 Eilenberg and Steenrod had utilised the categorial framework in giving an axiomatization of the homology and cohomology of a topological space. This eventually led to the delineation of the central notion of an 'Abelian' category. MacLane summarizes the ensuing developments

This idea [Abelian category], though formulated by MacLane in 1950, was not really effective until it was used by Alexander Grothendieck in his decisive 1957 paper "On some remarks on homological algebra." This paper not only summarized ideas of the previous period by presenting a definitive and "most general" formulation of homological algebra but it also initiated the categorical emphasis of the new period. In preparing it, Grothendieck apparently rediscovered the notion of an abelian category; moreover, he recognised the crucial new example that of the category of sheaves (modules) over a fixed topological space.

From this point, Grothendieck went on to wholly reorganise algebraic geometry in relentlessly conceptual form in which algebraic geometry appeared both in its own right and as the most general study of commutative rings. His influence was exercised through many seminars on algebraic geometry, for example, the well known SGA-4 (séminaire de Geometrie Algebraique du Bois Marie 1963/4) - privately circulated at that time and finally published in three volumes (Artin, Grothendieck, and Verdier, 1972-73).

Grothendieck's work was a direct sequel to the influence of Bourbaki. Bourbaki had organised the exposition of mathematics by putting the right general concepts first. Grothendieck turned ths systematic use of generality into an explicit tool for research. [1981 p.25]

Put boldly, Grothendieck showed that 'topology was pointless'. In other words, it is not the points of the space that are important in the study of the topological properties of a topological space X but rather the structure of its open sets. For all intents and purposes, e.g. the cohomology of X, the category of sheaves over X, Sh(X), may be substituted for X and Sh(X) is defined in terms of the open sets and coverings of open sets. Moreover these categories were seen to be of considerable importance in their own right. However, for further topics in algebraic geometry Grothendieck formulated a more general notion of a topology ("Grothendieck topologies") and its sheaves. [See Chapter 5] Categories of such sheaves were called toposes or "Grothendieck toposes". In the theorem bearing his name, Giraud, a member of Grothendieck's seminar, gave necessary and sufficient conditions characterizing this class of categories. It was Lawvere and Tierney's modification of Giraud's axioms within first order logic that constituted the axioms for an elementary topos.

iii.b) Lawvere's work and....

Let us now turn to Lawvere's categorial analysis of set theory. Since his doctoral thesis in 1963 Lawvere's central interest has been the foundations of mathematics. More specifically he has been concerned with founding mathematics upon categorial notions i.e morphisms and composition of morphisms. His attitude to the foundations provided by Zermelo-Fraenkel set theory may be gleaned from the following passage

When the main contradictions of a thing have been found, the scientific procedure is to summarize them in slogans which one then constantly uses as an ideological weapon for the further development and transformation of the thing. Doing this for "set theory" requires taking account of the experience that the main pairs of opposing tendencies in mathematics take the form of adjoint functors, and frees us of the mathematically irrelevant traces (ϵ) left behind by the process of accumulating (U) the power set (P) at each stage of a metaphysical "construction". [Lawvere 1970 p.329]

This attitude was already evident in the spirit of Lawvere's seminal paper 'An elementary theory of the category of sets' which he describes as follows

We adjoin eight first-order axioms to the usual first-order theory of an abstract Eilenberg-MacLane category to obtain an elementary theory with the following properties: (a) There is essentially only one category which satisfies these eight axioms together with the additional (nonelementary) axiom of completeness, namely, the category $\underline{S}$ of sets and mappings. Thus our theory distinguishes $\underline{S}$ structurally from the other complete categories, such as those of topological spaces, groups, rings, partially ordered sets, etc. (b) The theory provides a foundation for number theory, analysis, and much of algebra and topology even though no relation ϵ with the traditional properties can be defined. Thus we seem to have partially demonstrated that even in foundations, not Substance but invariant Form is the carrier of the relevant mathematical information. [Lawvere 1964 p.1506]

In this passage Lawvere refers to 'completeness' i.e. "complete categories". This notion, which I shall presently explicate, lies at the heart of the categorial account of set theory. Roughly speaking, the basic set operations e.g. cartesian products and intersections, are generalized within complete categories. This work was subsequently utilized in the formulation of elementary topos theory and in this respect contribute to MacLane's comment that "The axioms for a topos depend on [an]..understanding of the 'Universal' properties of the basic constructions of set-theory" [1979 p.1008]. Furthermore, as we shall see, completeness is also intimately connected to the link provided by Giraud's axioms between Lawvere's programme, the formulation of elementary topos theory and Grothendieck's categories of sheaves.

One feature of a complete category, then, is that it contains all products in the sense described above. It was stated that a product is a particular example of a 'limit'. Now a complete category is a category displaying all 'limits of diagrams'. These also highlight the ubiquitous use of commutative diagrams and the 'proof' technique of 'diagram chasing'. [See MacLane 1982] Let me explain.

First, the *formal* notion of a diagram for a category C. This is defined to be a collection of C-objects (the 'vertices') together with a collection of C-arrows between some of them. Where the diagram is of reasonable size it can be represented schematically in the usual way and such representations, of course, gave rise to the present notion which includes infinite diagrams. Let Δ be a C-diagram with vertices

$\{D_i : i \in I\}$. A *cone* over Δ is a collection of C-arrows $\{f_i : A \to D_i : i \in I\}$ for a fixed object A (the vertex of the cone) such that for any arrow $f: D_i \to D_j$ the following commutes

$$\begin{tikzcd} & A \arrow[dl, "f_i"'] \arrow[dr, "f_j"] & \\ D_i \arrow[rr, "d"] & & D_j \end{tikzcd}$$

[diagram 3]

A limit for Δ ($\underline{\lim}\Delta$) is a cone $\{f_i : A \to D_i : i \in I\}$ such that for any other cone $\{g_i : B \to D_i : i \in I\}$ there is a unique arrow $B \to A$ making

$$\begin{tikzcd} B \arrow[rr, "g"] \arrow[dr, "g_i"'] & & A \arrow[dl, "f_i"] \\ & D_i & \end{tikzcd}$$

[diagram 4]

commute for each $i \in I$. Usually a limit is identified with its vertex. The following examples will illustrate how many of the constructions in the category of sets are realized in terms of limits.

EXAMPLE 1: Let Δ be a diagram consisting of two objects A,B and the empty collection of arrows. In this case Δ turns out to be the product of A and B, i.e $\underline{\lim}\Delta = A \times B$.

EXAMPLE 2: Let Δ be the diagram consisting of objects A,B,C and arrows $f: A \to C$ and $g: B \to C$. The limit of such a diagram is called the 'pullback' of arrows f,g for reasons which will become evident. The pullback is usually schematized as the commutative diagram

$$\begin{tikzcd}
\mathrm{Lim}\,\Delta \arrow[r, "f'"] \arrow[d, "g'"'] & B \arrow[d, "g"] \\
A \arrow[r, "f"] & C
\end{tikzcd}$$

[diagram 5]

By looking at the pullback in the category of sets S we can get the idea of the constructions a pullback encapsulates. To begin with the general form is $\underline{\lim}\Delta=\{\langle x,y\rangle\in A\times B: f(x)=g(y)\}$. This gives rise to some interesting particular cases; if C is a singleton, then $\underline{\lim}\Delta=A\times B$ (up to isomorphism!); if $A\subseteq C$ and f an injection, then $\underline{\lim}\Delta=g^{-1}[f[A]]$; and if $A,B\subseteq C$ and f,g are inclusion maps, then $\underline{\lim}\Delta=A\Delta B$.

EXAMPLE 3: Let Δ be the empty diagram. Then a cone for this diagram is simply a C-object. Thus $\underline{\lim}\Delta$ is a C-object such that for any other C-object X there is only one arrow from X to $\underline{\lim}\Delta$. Such a limit is called a terminal object and denoted by 1_C but usually where unambiguous the subscript is omitted. In S the singleton $\{\emptyset\}$ is conventionally delegated the role of terminal object; though of course from the categorial standpoint any singleton will do. It is quite straightforward to demonstrate that a terminal object in a category is unique (again up to isomorphism). We can now perform one of those dizzy leaps characteristic of category theory. *All limits are unique up to isomorphism!* For some meditation on their characterization reveals them to be terminal objects in the category formed from their associated collection of cones.

Recall that the non-elementary axiom in Lawvere's characterization of the category of sets asserted completeness, that is, the existence of *all* limits. The examples given above were examples of finite limits, i.e. limits of finite diagrams. We shall see that in the presence of certain elementary conditions finite limits are deemed sufficient for the set theory in a topos. In fact in some circumstances only products and a terminal object yield all the finite limits. In any case Lawvere's higher-order axiom is obviated.

We might also at this point mention the connection with the theorem of Giraud characterizing Grothendieck toposes which led Lawvere and Tierney to the generalized set theory of elementary toposes. The key features of Giraud's characterization were couched in terms of 'exactness' properties of a category. (It also happened to be the case that Tierney had worked on the theory of exact categories.) Now, an exact category is defined to be a category with all finite limits and colimits (i.e. its corresponding opposite category has all finite limits.) Johnstone also remarks that

The starting point ..[of the second line of development of elementary topos theory].. is generally taken to be F.W.Lawvere's pioneering 1964 paper on the elementary theory of the category of sets [1964]. However, I believe that it is necessary to go back a little further, to the proof of the Lubkin-Freyd-Mitchell embedding theorem for abelian categories [AC]. It was this theorem which, by showing that there is an explicit set of elementary axioms which imply all the (finitary) exactness properties of module categories, paved the way for a truly autonomous development of category theory as a foundation for mathematics...[1977 p.xii]

In the above quotation from Lawvere's 1964 he mentions that his theory will provide a foundation for, among other things, number theory and analysis. For these disciplines classical set theory provides an axiom of infinity. The set of 'real numbers' can then be constructed by means of the power set operation on ω (or the set designated as the collection of natural numbers). Lawvere's ingenious categorial correlate of the axiom of infinity is the postulation of a 'natural number object'. This is an object $\boldsymbol{N}$ together with arrows $s: \boldsymbol{N} \to \boldsymbol{N}$, $\sigma: 1 \to \boldsymbol{N}$ (the category thus require a terminal object) such that for any arrows $h: X \to X$, $x: 1 \to X$ there is a unique arrow $f: \boldsymbol{N} \to X$ making the following diagram commute

$$
\begin{tikzcd}
1 \arrow[r, "\sigma"] \arrow[dr, "x"'] & N \arrow[r, "s"] \arrow[d] & N \arrow[d, "f"] \\
& X \arrow[r, "h"] & X
\end{tikzcd}
$$

[diagram 6]

This is an axiom of Lawvere's 1964 system. The notion of a topos is more general and does not incorporate this axiom. However, it is by means of its addition to elementary topos theory that Lawvere and Tierney formulated the continuum hypothesis within the categorial framework and as a consequence shed considerable light on Cohen's forcing method. Note also that this axiom allows further generality to the notion of a natural number.

> ...the foundations of the arithmetic of natural numbers can be lifted to any topos with a natural numbers object. The power of the axiomatic method, and the ability of abstraction to simplify and get at the heart of things will perhaps be brought home if one reflects that a "natural number"...might in fact be anything from a continuous function between sheaves of sets of germs (local homeomorphisms) to an equivariant mapping of monoid actions, or a natural transformation between set-valued functors defined on an arbitrary small category. [Goldblatt 1979 pp334-5]

And looking ahead to applications in analysis, Lawvere in his 1970 observed

> In any topos satisfying (ω) [presence of natural number object] each definition of the real numbers yields a definite object, but it is not yet known what theorems of analysis can be proved about it." [p.334]

Lawvere's paper of 1964 axiomatizing the category of sets laid much of the groundwork for the formulation, some five years later, of the elementary theory of a topos. There it was merged with the sheaf theory of the Paris school of algebraic geometry. However, on completing this paper Lawvere was not satisfied with its foundational viability. In fact the last words of this paper read: "it is the author's feeling that when one wishes to go substantially beyond what can be done in the theory presented here, a more satisfactory foundation will involve a theory of the category of categories." [p.1510] Lawvere produced such a theory in his 1966 paper entitled "The Category of Categories as a Foundation for Mathematics". For various reasons this theory too proved unsatisfactory. [See, e.g. Isbell 1967] But Lawvere had also pursued his interest in the logical aspects of the category of sets and related categories, and it was this line of investigation that provided the essential link with axiomatic sheaf theory and consequently elementary topos theory.

iii.c)....and the development of elementary topos theory.

In their papers introducing elementary topos theory Lawvere and Tierney characterized the notion as a category which is; (1*) finitely complete; (2*) Cartesian closed; and has (3*) a subobject classifier. In this section I shall give an account of the development of elementary topos theory in terms of, and explicating, the postulates here given. [(1*) has already been addressed on the discussion of the role of exactness.] Since the original axiomatization there have been quite a few different equivalent sets of axioms presented in the literature. First of all, some have arisen as a result of simplifications, i.e. as in the case of many important axiomatizations e.g. the propositional calculus of *Principia Mathematica*, certain conditions have been found redundant. Second, the particular axioms used usually reflect the purposes in hand, e.g. whether they are being used for work within algebraic geometry or foundations. All this reflects the dual origins and reinforces the scope of the theory. For "A topos is, from one point of view, a category with certain properties characteristic of the category of sets...From another point of view, a topos is an abstraction of the category of sheaves over a topological space." [Barr/Wells 1985 p.64]. One of the advantages of these dual roots is a mutually beneficial exchange of concepts, methods and results between set theory and algebraic geometry. I shall basically concentrate on the presentation given above and on the version that is associated with LST. This latter presentation also serves to emphasize topos theory *qua* generalized set theory.

I begin with cartesian-closed categories. I shall understand a category to be cartesian-closed if it is finitely complete and has exponentials. (Axiom (2*) is referring to the addition of exponentials) Thus a topos is a cartesian-closed category satisfying (3*). In set theory, for arbitrary sets X, Y there exists a set X^Y which is the set of all functions from Y to X , ie hom(Y, X). Of course, for any category C and C-objects A, B; hom(A, B) is the collection of morphisms from A to B, but is, loosely speaking, only an object of the ambient category C if C is the category of sets. Exponentials are the categorial correlate, or external characterization, of this set-theoretic construction i.e. X^Y [=hom(Y, X)] for an arbitrary category. Thus we may think of them as 'internal hom-sets'. The external characterization is motivated by two observations on functions related to sets of the form X^Y. First to each such set there is an associated *evaluation* function $ev\colon X^Y \times Y \to X$ defined by $ev(\langle f, y\rangle)=f(y)$ for $f \in X^Y$, $y \in Y$. Second there is a natural bijection $hom(Z\times Y, X) \cong hom(Z, X^Y)$ induced by the universal property of *ev* with respect to members of $hom(Z\times Y, X)$. Namely, given $g\colon Z\times Y \to X$ there is a unique $\hat{g}\colon Z \to X^Y$ such that $ev(\hat{g}(z), y)=g(z, y)$ for all $z \in Z$, $y \in Y$. Furthermore for $h \in hom(Z, X^Y)$ it is straightforward that there exists $g \in hom(Z\times Y, X)$ such that $h=\hat{g}$. Thus by analogy we say a category C (with products) satisfies (2*) if for arbitrary C-objects a, b, c there exists a C-object b^a and C-arrow $ev\colon b^a \times a \to b$ such that given C-arrow $g\colon c\times a \to b$

there is a unique C-arrow $\hat{g}: c \to b^a$ making the following diagram commute.

$$\begin{tikzcd} b^a \times a \arrow[d, "\hat{g} \times id_a"'] \arrow[dr, "ev"] & \\ c \times a \arrow[r, "g"'] & b \end{tikzcd}$$

[diagram 7]

commute. Note that this condition may readily be presented in terms of the existence of certain adjoints, roughly, functors $(-\times a): C \to C$ have right adjoints . A topos itself may be thought of as a category that supports certain adjoints. In fact this is perhaps the most categorial or generalized approach and again underlines the conceptual power of adjointness.

Before proceeding to a discussion of the formulation of axiom (3*), in the nature of a bridging passage, it is apposite to say a few words on Lawvere and cartesian closed categories. Lambek and Scott in their 1986 credit the invention of cartesian closed categories to Lawvere in his 1964. Whereupon soon after in 1966 Eilenberg and Kelly published an extensive article on the little more general but closely related class of categories displaying internal hom-sets, namely, 'closed categories'. Although Lawvere doesn't refer explicitly to cartesian closed categories or develop the notion as such in his 1964 paper it is certainly the case that any category satisfying his first two axioms is cartesian closed. There are certain further connections that should be mentioned.

Prior to, and subsequent to, his work on the category of sets Lawvere had worked on a categorial account of algebraic logic [see Lawvere 1963, 1965]. In this work categories were employed to represent theories, which in itself prefigured important developments, including, as we shall describe below, LST itself. Moreover this work is important in that it saw the beginnings of Lawvere's investigations of the internal representation of logical concepts, including comprehension. More specifically, in his 1965 Lawvere had generalized his categorial account of finite algebraic theories to that of elementary theories. These required a representation of partially defined operations and relations. For this purpose an 'object of truth-values' was added to the category *qua* theory. The idea of a category with such an object was to play a vital part in subsequent developments.

In his 1963 substitution was treated as composition of arrows. This device in fact was standard in the λ-calculus ('theories of functionality'). As is made manifest in the work of Lambek and Scott [1984, 1986] typed λ-calculii may be identified with cartesian closed categories or as they put it "Both are attempts to describe axiomatically the process of substitution, so it is not surprising to find that these two subjects are essentially the same." [1986 p.41]

In his 1965 Lawvere introduced quantifiers in terms of adjoints. We see in this work an early formulation of categories with 'subobject' correlates. These Lawvere called 'elementary doctrines'. Following on from these his foundational program progressed with the development of 'hyperdoctrines' in his 1969a as well as a paper entitled 'Diagonal arguments and cartesian closed categories' [1969b]. Hyperdoctrines were explicitly given as a subclass of cartesian closed properties together with some internal logical characteristics including an associated power set correlate for each object (he called the objects 'types' and "for each type X there is a cartesian closed category P(X) of 'attributes of type X'"). Thus, these in certain key respects anticipated topoi. Moreover, in his 1970a Lawvere informs us that

> The notion of hyperdoctrine was introduced (*Adjointness in Foundations*, to appear in Dialectica) in an initial study of systems of categories connected by specific kinds of adjoints of a kind that arise in formal logic, proof theory, sheaf theory..... Since then the author has noticed that yet another "logical operation", namely that which assigns to every formula ϕ its "extension" $\{x:\phi(x)\}$ is characterized by adjointness... The second part of this article is devoted to a preliminary discussion of this sort of adjoint which we call tentatively the Comprehension Schema. [p.1]

The above work on the categorial comprehension schema was facilitated by Lawvere's treatment of a two element set, canonically $\{\emptyset,\{\emptyset\}\}$ or 2, as an object of truth-values, i.e. {*false,true*} in S the category of sets. This usage is reminiscent of Frege's higher-order logic where truth-values were assumed to be objects and concepts propositional functions taking these objects as values. In Lawvere's 1964 system an arrow with codomain 2 was understood to be the characteristic function of a subset. However, Lawvere's categorial definition of 'subset' was as follows: a is a subset of A iff a is a monomorphism with codomain A. Subsets, then, were treated as certain kinds of arrows. Another important external characterization made by Lawvere in this paper was that of an 'element'. This in fact is given as the first definition of the paper. An element of A is an arrow with codomain A and domain the terminal object 1. One feature of his system that Lawvere points out is that not only every arrow with domain 2 is the characteristic function of some 'subset' but conversely, every 'subset' has a characteristic function. However, in this system although subsets could throughout be treated in terms of their characteristic functions, as MacLane observes "this proved quite difficult to handle" [1975 p.123]. Amongst other things (3*) was to simplify this procedure.

From the mid-sixties progress was made on two fronts. First, in connection with general exactness properties of categories which was brought to fruition with Barr's explicit formulation of 'Exact category' in his paper 'Exact categories and categories of sheaves' [1971]. Secondly, on the internalization of logical concepts. In particular Lawvere and Tierney who had in 1969 embarked on the study of axiomatic sheaf theory had investigated the results of postulating the existence of a generalized truth-value object Ω within a category. For it was observed by Lawvere that not only were categories of sheaves characterized by the exactness properties as discussed above but also offered interpretations of the logical constructions generalized from his work on categorial set theory. More specifically

> he observed that every Grothendieck topos has a truth value object Ω, and that the notion of Grothendieck topology is closely connected with endomorphisms of Ω [Johnstone 1977 p.xv]

The upshot was (3*), sometimes referred to as the Ω-axiom, which was the final postulate characterizing elementary topos theory. Before proceeding we make two observations on the merger of categorial set theory and axiomatic sheaf theory in respect of the idea of finding an elementary characterization. First, a general comment by Gray

During the 1960's Lawvere was working on two related questions, the categorical descriptions of *theories* and of *sets*.

...in Lawvere [1964] there is an account of the elementary theory of the category of sets. This obviously suggests looking for the elementory theory of other categories. Lawvere described categories themselves [*], and Schlomiuk described topological spaces [**], but neither of these descriptions was completely satisfactory. However Bunge [1966] treats categories of set-valued functors in an interesting and useful way......It seems now, and it seemed then, very natural to ask for a description of the elementary theory of categories of sheaves. Giraud's theoremms characterising Grothendieck topoi were known, but they were not elementary, and depended heavily on set theory.

It was a brilliant inspiration to see that the answers to these two questions were the same: an elementary theory [***] was slightly generalised to a cartesian closed category with a subobject classifier (= truth value object) thereby giving equally well the appropriate elementary notion of a category of sheaves. [1977 pp.62-63. * 'The category of categories as a foundation for mathematics' 1966. ** 'An elementary theory of the category of elementary spaces' 1970. *** In the sense of Lawvere 1965]

Secondly, Lawvere and Tierney had studied the model constructions of Robinson for non-standard analysis and Cohen's for independence proofs and as Lawvere says "It was these examples that led Tierney and me to further generalize the previous theory of topoi in 1969 by making it elementary". [1976 p.104] Lawvere judged that within the framework of sheaf constructions the ideas underlying Robinson's and Cohen's constructions "can be dealt with in perhaps more natural and certainly more invariant fashion... " [1976 p.104] But there was a problem. These constructions involved moving from ground models which were Grothendieck toposes to models of the required elementary theories of analysis and set theory which turned out *not* to be

Grothendieck toposes. Lawvere and Tierney detemined that an elementary theory of topoi would resolve this tension.

We now turn to the formulation of the Ω-axiom. This axiom is, Lawvere suggests, the categorial correlate of the restricted comprehension principle of *ZF*. We will consider this aspect of it in greater depth in Chapter 4. For the present to give an introductory account of it we first make some observations with respect to classical set theory. First, for reasons that will become apparent, we call the function from 1 into 2 with value 1 '*true*'. Let X, Y be sets such that $X \leq Y$ and $f:X \to Y$ the inclusion map. Then there is a unique map $\chi_f:Y \to 2$ such that the following diagram

$$\begin{tikzcd} X \arrow[r, "f"] \arrow[d] & Y \arrow[d, "\chi_f"] \\ 1 \arrow[r, "true"] & 2 \end{tikzcd}$$

[diagram 8]

commutes. Furthermore, it is readily seen that the above diagram has the properties of a pullback. This situation also holds over for arbitrary sets X, Y where $f:X \to Y$ is an injection. In that case, from the categorial point of view χ_f is the characteristic function of X just as much as it is of f[X]. In fact since a basic idea of category theory is to give external characterizations of concepts interest focuses on the relevant arrows, *true*, f, and χ_f.

Since in category theory we deal in arrows rather than functions to generalize the above discussion for arbitrary categories we need a correlate for injective functions. This is yielded by the following characterization of injective functions in terms of their relations to other functions: a function f is injective iff for any functions g, h if $f \cdot g = f \cdot h$ then $g = h$; i.e. 'left-cancellable'. The correlate then is called a 'monic' (from monomorphism) and an arrow is defined to be a monic iff it is left-cancellable. This definition is conservative with respect to those categories where arrows correspond to functions. But clearly not in their corresponding opposite categories. Or, for example, in a partially ordered set *qua* category where *every* arrow is monic.

A finitely complete category (or equivalently a category with terminal object and pullbacks) satisfies (3*) if it has an object Ω and a map *true*: $1 \to \Omega$ such that for each monic $m: A \to B$ there is a *unique* arrow $\chi_m: B \to \Omega$ such that the following diagram is a pullback.

$$\begin{tikzcd}
A \arrow[r, "m"] \arrow[d] & B \arrow[d, "\chi_m"] \\
1 \arrow[r, "true"] & \Omega
\end{tikzcd}$$

[diagram 9]

*

Using the presence of the subobject classifier the logical operations can all be formulated internally, i.e. in terms of arrows in the category. It will be useful to have a brief look at this internal logic.

In classical set theory the collection of subsets of a given set can be construed algebraically. In fact it is a Boolean algebra under the relation of subset inclusion. Furthermore, since subsets may be identified with their characteristic functions, this collection is also a Boolean algebra under the appropriate relation and, clearly, is isomorphic to the algebra of subsets. In this case the object of truth values is the set 2, i.e. $\{\emptyset, \{\emptyset\}\}$, and is itself a Boolean algebra. It is not difficult to see that the structure of the algebra of truth values is a crucial factor in determining the behaviour of subsets. (In fact, as we shall see, Zermelo's appeal to 'definite properties' is made in order to ensure that classical two-valued logic obtained in his set theory.) Now, in general, the algebra of truth-values, that is, the algebraic structure of Ω, is a *complete Heyting algebra*. And as in classical set theory the structure of Ω and the 'subsets' of a given object are intimately related. This follows directly from axiom (3*).

In general the subsets of an object in a topos have the structure of a complete Heyting algebra just as does the structure of Ω. A topos is called Boolean if the algebra of subsets of each object in the topos is Boolean. But this property is completely determined by the structure of Ω. A topos is Boolean iff Ω is Boolean. [The intimate connection between subsets and the internal logic is made manifest in generalized set theory. See Chapter 4 on topos theory and definite properties.]

Logical operations such as negation, conjunction, disjunction and implication are explicable as arrows $\Omega \to \Omega$ or $\Omega \times \Omega \to \Omega$. I shall look at the identification of the arrows carrying negation and conjunction in a topos. First we have to develop a little more machinery. As well as a terminal object 1 it is provable that every topos E contains an initial object 0, i.e. an object such that given any E-object X there is a unique E-arrow $0_X: 0 \to X$. In classical set theory the terminal object is simply the empty set. Notice also that in the opposite category constructed from E the initial object is the terminal object in E. We can now define the companion to *true*: $1 \to \Omega$, namely: *false*: $1 \to \Omega$. We have the arrow $0_1: 0 \to 1$ which, as is not difficult to show, is a monic and *false* is defined to be its characteristic arrow as in the following diagram.

$$\begin{tikzcd}
0 \arrow[r, "0_1"] \arrow[d] & 1 \arrow[d, "false"] \\
1 \arrow[r, "true"] & \Omega
\end{tikzcd}$$

[diagram 16]

The arrow *false*: $1 \to \Omega$ is itself a monic (as is any arrow with domain 1) and thus also has a characteristic arrow, which we denote $\#: \Omega \to \Omega$.

$$\begin{tikzcd}
1 \arrow[r, "false"] \arrow[d] & \Omega \arrow[d, "\#"] \\
1 \arrow[r, "true"] & \Omega
\end{tikzcd}$$

[diagram 17]

$\#:\Omega\to\Omega$ is a truth function and in fact is the 'negation' arrow. This is clear if we consider the classical case where $\Omega=2$.

We now turn to conjunction. Since a topos has products, we have in particular the product $\Omega\times\Omega$. It follows from the fact that a product is a limit for a pair of arrows, in particular if each of the pair is *true*: $1\to\Omega$ then their is a unique arrow $\langle true, true\rangle: 1\to\Omega\times\Omega$ such that the following diagram commutes

$$\begin{tikzcd}
 & 1 \arrow[dl, "true"'] \arrow[d, dashed, "\langle true{,}true\rangle"] \arrow[dr, "true"] & \\
\Omega & \Omega\times\Omega \arrow[l] \arrow[r] & \Omega
\end{tikzcd}$$

[diagram 18]

We now identify the conjunction arrow $\&:\Omega\times\Omega\to\Omega\times\Omega$ as the characteristic arrow of $\langle true, true\rangle: 1\to\Omega\times\Omega$. Again, this is clarified if one considers the classical case.

Not only does topos theory shed light on issues with regard to what I have called the mathematical approach to set theory; one of the interesting aspects of topos theory, and indeed one of its foundational bonuses, is that it also illuminates issues directly concerning the logical approach. [Recall that in this approach an attempt is made to provide a logical analysis of the concept of 'set' and in the case of the logicists to perform a reduction.] A notable example of how topos theory provides insight into the relationship between logical concepts arises in connection with the axiom of choice.

The axiom of choice has been generally regarded as the principle characteristic of classical mathematics. This axiom can be formulated within set theory by the proposition that for every surjective function $f:X\to Y$ there is a function $g:Y\to X$ such that for all $y\in Y$ $f\circ g(y)=y$. The correlate of a surjective function in category theory is an 'epic' arrow, which is an arrow that is right cancellable. The above form of the axiom of choice is directly translatable within topos theory as follows: (AC) - 'for each epic $f:X\to Y$ there is an arrow $g:Y\to X$ (provably monic) such that $f\circ g=id_Y$. Diaconescu in his 1975 proved the surprising theorem that if a topos satisfies (AC) then it is Boolean. Regarded as a site of mathematical activity, a Boolean topos is essentially an arena of classical mathematics.

**

In the above discussion we have mentioned subsets and some of the machinery for their categorial representation. In set theory perhaps one of the most mathematically important and controversial axioms is that which allows the collecting up of *all* subsets of an arbitrary set, i.e. the power set axiom. So far this key feature of the category of sets has not been explicitly addressed in connection with topoi. However, it was mentioned that the addition of a natural number object facilitates not only categorial arithmetic but analysis. This latter, in classical mathematics requires, at least in so far as it is set theoretically based, a power set operation. Clearly, if topos theory is to have any foundational viability with respect to extant mathematics it must have at least an equally powerful operation to, or correlate for, the power set construction. In fact this has already been included in the topos axioms much in the same way as Lawvere incorporated the power set axiom in his elementary theory of the category of sets, namely, via the construction of exponents upon the object of truth values.

Consider a set X. There is a bijection between subsets of X and characteristic functions defined over X. Furthermore there exists the exponential 2^X which is the collection of these characteristic functions. Let P(X) denote the power set of X. Then it is straightforward that the bijection between subsets of X and their characteristic functions induces a bijection between P(X) and 2^X. Now let E be a topos and X an E-object. Accordingly then, E contains the exponent Ω^X. Exponents with base Ω are called power-objects (strictly speaking a pair including the corresponding evaluation arrow i.e. $\langle\Omega^X, ev_X\rangle$) and these are the categorial correlates of power sets in a general category.

It turns out that the following (elementary) conditions constitute an equivalent axiomatization of a topos. (1') terminal object; (2') products; (3') subobject classifier; (4') power objects. This axiomatization most closely reflects the presentation of the theory LST which will be discussed in Chapter 4. As I have stated, topos theory is generalized set theory and LST is topos theory in its optimum form *qua* generalized set theory. Having given a basic account of the relevant concepts of category theory and topos theory we are now in a position to retrace our steps, returning to a discussion of

ZF and Cohen's independence results, underlining the connections with the above developments and showing why LST is the next step in the evolution of set-theory in the context of the mathematical approach.

THE ALGEBRAIC VIEW OF SET THEORY.

Topos theory is a generalization of the set concept that constitutes the informal theory formalized by ZF. The process of generalization is, of course, a fundamental conceptual tool throughout mathematics (an example of particular interest we describe is the case of the generalization of 'sheaf over a topology' for arbitrary toposes. See Chapter 5). Generalization is particularly entrenched in the algebraic tradition. Now not only is the generalization of set theory into topos theory indicative of the adoption of an algebraic view of set theory; to a great extent, this was the case from Zermelo onwards.

In the last chapter we discussed the emergence and development of category theory from the background of the growth of algebra, set theory and a general emphasis on structural considerations. We may also add that the rise of algebra in the second half of the nineteenth century, e.g. non-commutative algebras, has often been overlooked or underestimated as a source of the modern foundational studies initiated around the turn of the century. An essential ingredient in the pattern of the aforementioned growth has been the steadily increasing and significant contribution from formalization and set theory. However, the mathematical approach to set theory, the details of which were addressed in Part II, based as it was on Zermelo's work, is itself (and was from its inception) embedded in this growth and partakes of the treatment accorded therein to a general mathematical theory.

Put in another way: in Part II I have essentially described the initial and crucial stages of the successive axiomatizations and formalization of set theory, within the mathematical approach, into its present incarnation i.e. ZF. [Recall that the pivotal point stressed was the first-orderization of set theory by Skolem.] Now the overall development of set theory is to be interpreted as the evolution of an algebraic concept such as 'group' or 'topological space'; (acknowledging, of course that set theory has a foundational role i.e. is *applied*, and incorporates, as it was specifically designed to do, a theory of transfinite arithmetic). Furthermore, Cohen's results emphatically underpin the adoption of a relativistic account of the set concept.

The shift, then, from ZF set theory to LST i.e. topos theory is a natural step in the evolution of the set concept. For, stated bluntly, *the nature of the shift from ZF to topos theory is revealed through the recognition that the enterprise of formalizing set theory within the mathematical approach bears all the hallmarks of the development and formalization of an algebraic concept, or concept algebraically construed. This shift is a process naturally occuring in the development of algebraic concepts, namely:* generalization.

More prosaically perhaps, the shift takes us to a more appropriate richer class of models or 'sites of mathematical activity'. Lawvere and Tierney, the inventors of elementary topos theory, were very much concerned with models of set theory as sites of mathematical activity. This concern was in fact evident throughout the mathematical approach starting with Zermelo; and certainly Fraenkel, Skolem and von Neumann's approach focused on these models. In particular they understood the axioms as providing an implicit definition. This view which is fundamental within the algebraic approach to mathematical concepts was emphasized, as we have stated, in Hilbert's early work and as we shall highlight, greatly influenced Zermelo amongst many others. We have already mentioned its influence on the founders of abstract algebra. Von Neumann, who had a foot both in the set theorists and abstract algebraists camps, was also thus influenced. Hilbert's pioneering work with models in connection with axiomatics is stressed by Weyl. Weyl writes:

It is one thing to build up geometry on sure foundations, another to enquire into the logical structure of the edifice thus erected. If I am not mistaken, Hilbert is the first who moves freely on this higher "metageometric" level: systematically he studies the mutual independence of his axioms and settles the question of independence from certain limited groups of axioms for some of the most fundamental geometric theorems. His method is the *construction of models*: the model is shown to disagree with one and to satisfy all other axioms; hence the one cannot be a consequence of the others. One outstanding example of this method had been known for a considerable time, the Cayley-Klein model of non-Euclidean geometry. For Veronese's non-Archimedean geometry Levi-Civita (shortly before Hilbert) had constructed a satisfactory arithmetical model. The question of *consistency* is closely related to that of independence. The general ideas appear to us almost banal today, so thoroughgoing has been their influence upon our mathematical thinking. Hilbert stated them in clear and unmistakable language, and embodied them in a work that is like a crystal: an unbreakable whole with many facets. Its artistic qualities have undoubtedly contributed to its success as a masterpeice of science. [1972 p.265]

I do not propose to define algebra or the algebraic approach. To begin with 'algebra' and the 'algebraic approach' just like 'game' in Wittgenstein's well known example is a matter of family resemblances and we would be both mutilating and unduly constraining these notions by offering a definition. In fact MacLane in his retiring address as President of the American Mathematical Society made the following response to the question "What is Algebra?"

> On some occasions I have been tempted to try to define what algebra is, can, or should be....But no such formal definitions hold valid for long, since algebra and its various subfields steadily change under the influence of ideas and problems coming not just from logic and geometry, but from analysis, other parts of mathematics, and extra mathematical sources. The progress of mathematics does indeed depend on many interlocking, unexpected and multiform developments. [MacLane 1976 p.36]

However, I shall bring out and comment on certain central features of the development of set theory to both clarify and give substance to the claim that this development is soundly characterized by the term 'algebraic approach'. Put crudely then, starting with Zermelo, set theory was developed and treated much the same way as, for example, the notion of a group. It is the key aspects of this *treatment* that I discuss in this chapter. (But before proceeding it should be said that in the axiom of separation and the question of definite properties, a notion of special importance for axiomatic set theory, there is something of a 'residual logic input' perhaps not immediately apparent in say the power set axiom or axiom of unions and perhaps not at all in systems such as the axioms of group theory. This is not surprising since the axiom of separation partly originates from the comprehension principle. Furthermore it is not surprising that its categorial manifestation involves the source of internal logic in a topos, namely: the subobject classifier. This topic is taken up in the following chapter.)

In laying out the axioms of his set theory Zermelo's idea was that he was providing an implicit definition of a concept in much the same way that an algebraist views the formulation of axioms for groups, rings etc. This is acknowledged in Skolem's first remark where, for example, he states that

it seems to be clear that, when founded in such an axiomatic way, set theory cannot remain a privileged logical theory; it is then placed on the same level as other axiomatic theories. [1922 p.292]

Recall that Zermelo's 1908a paper opens with the declaration

Set theory is that branch of mathematics whose task is to investigate mathematically the fundamental notions "number", "order", and "function"...it constitutes an indispensable component of the science of mathematics. [p.200]

And note that he begins section §1 - the main body of his 1908a paper - by stating:

Set theory is concerned with a *domain B* of individuals, which we shall call simply *objects*, and among which are the *sets* [p.201]

The first quotation establishes unequivocally that his axioms are to be the axioms of a *mathematical* theory. In fact his two papers dated 1908 are replete with passages emphasizing this point. For example, in criticizing Peano for not making the distinction between 'set' and 'class' he claims

as I shall soon show elsewhere [1908a], those who champion set theory as a purely mathematical discipline that is not confined to the basic notions of traditional logic are certainly in a position to avoid, by suitably restricting their axioms, all antinomies discovered until now. [1908 p.189]

Zermelo's attitude sometimes became nakedly pragmatic. Faced with the challenge of the Burali-Forti antinomy upon the theory of well-ordering he does not cling to any fixed notion of set but takes the position that

....the solution of these difficulties is not to be sought in the surrender of well-ordering but only in a suitable restriction of the notion of set. [1908a p. 191]

The opening sentence of section §1 is certainly not explicit so far as the domain *B* is concerned rather than a parameter it may be a reference to a fixed domain. But Zermelo worked within Hilbert's approach to axioms of mathematical concepts and Hilbert's attitude is straightforwardly revealed in the following comment on the real-number concept

In the theory of the number-concept the axiomatic method takes the following form:

We think of a system of things; we call these things numbers and denote them by *a*, *b*, *c*, ...We think of these numbers as standing in certain mutual relations, the exact and complete description of which is given by the following axioms...[Hilbert 1900 pp. 257-8 translation in Hallett 1984]

Responding to the above Hallett states

This was certainly Zermelo's position towards sets in his [1908b]: sets are just things in a domain, he says, about which the seven axioms he gives holds...It is curious that this position is close to Cantor's description of what happens when a new concept is introduced into mathematics..[1984 303-4]

Furthermore Hilbert's profound influence on Zermelo at this time is readily appreciated from the description below given by Zermelo, who, it must be remembered, up till his arrival in Göttingen had worked in mathematical physics

Thirty years ago, when I was a *Privatdozent* at Göttingen, I came under the influence of D. Hilbert, to whom I am surely the most indebted for my mathematical development. As a result I began to do research on the foundations of mathematics, especially on the fundamental problems of Cantorian set theory, whose true significance I learned to appreciate through the fruitful collaboration of the mathematicians at Göttingen. [p.1 of an unpublished report from Zermelo's *Nachlass*. Translation from Moore 1982 p.89. Incidentally Moore's interpretation takes the strong form "Under Hilbert's influence, which Zermelo later described as the most important of his mathematical career.." p.105]

We might also add that in his work of the late twenties Zermelo explicitly referred to a multiplicity of domains for his axioms. There was no question there, for him, of a fixed domain.

However, whilst holding with the above, it would be an error to claim that his view that axioms are to be understood as an implicit definition constituted a component of a fully worked out philosophical theory on his part (certainly not in 1908). Nor for that matter was this fully thought through at that stage by Hilbert. Hilbert's significant philosophical work i.e. the 'Metamathematics' followed in the wake of the intuitionist's attacks on classical mathematics. It was more the case that the view marked a *tendency* and attitude towards the process of axiomatization. However it was a tendency amongst succesive set theorists that was to be evident from that time onwards, through the work of von Neumann, up to the post-Cohen work of Lawvere and Tierney. (We have already noted this viewpoint in connection with the development of algebra - in particular the school of abstract algebra originating in Göttingen. Note also Lawvere's comment 'the essential role of theories is to describe their models' [1975 p.4])

With this attitude towards the axioms of set theory, just as in the case of group, the focus is upon models of the axioms. In group theory we are interested in the model (up to isomorphism) not in its elements ('groupies'?). Likewise 'universe of sets' takes centre stage rather than 'set'. Furthermore there is no relevant distinction to be made between isomorphic models. The 'points' cease to matter and the scene is set for a categorial account.

The above tendency within the mathematical approach is confirmed by von Neumann in his 1925. Russell, König, Weyl and Brouwer are grouped together as 'radical' with respect to their proposals *vis a vis* foundations, whilst

The other group, Zermelo, Fraenkel and Schoenflies, has eschewed so radical a revision. The methods of logic are not criticized to any extent, but are retained; only (the no doubt useless) naive notion of set is prohibited. To replace this notion the axiomatic method is employed; that is, one formulates a number of postulates in which , to be sure, the word "set" occurs but without any meaning. Here (in the spirit of the axiomatic method) one understands by "set" nothing but an object of which one knows no more and wants to know no more than what follows about it from the postulates. [p.395]

(Upon reading some of the works of the set theorists mentioned above it is certainly the case that one may find passages either implicitly or explicitly seemingly at variance with the algebraic view of set theory. For example, in Zermelo's writings, strong 'realist' leanings are identifiable and von Neumann influenced by the later Hilbert veers towards that brand of formalism. However, it is still the case that at least their *treatment* of the theory accords with the algebraic view. More recently where the professed metaphysical views clashed directly as in Gödel's platonistic philosophy the axioms were basically handled in the same manner as the axioms for groups. Furthermore, while waiting for an intuition informing us as to whether a particular proposition holds in the 'real' universe of sets in the meantime it is still incumbent on a realist to offer a response to Cohen's results. Or put another way, the arguments for topos theory may be applied with respect to 'sites of mathemematical activity' within some 'real' higher universe)

For the remainder of this section I want to mention three further details underlining the algebraic view and treatment of axiomatic set theory. First, briefly, an insight of Hallett's in his 1984 concerning Zermelo's reductionism and the emphasis on structure. He remarks in a footnote that

It is interesting to note that the positions of Bourbaki and other modern structuralists are strikingly reminiscent of the Zermelo-Hessenberg-Fraenkel position. For example, one might consider category theory as concerned not so much with objects but rather with isomorphisms and classes of isomorphisms. [p.249]

The position that prompts this remark is Zermelo's brand of reductionism, more specifically, the treatment of the reduction of numbers to sets. The now familiar approach is that of von Neumann where, for example, the natural numbers are identified with finite ordinals. On the other hand

The basic idea of the Zermelo (and Hessenberg) treatment of number is somewhat similar to that of Frege. As with Frege and Cantor the relations of equinumerosity and isomorphism between sets are taken to be fundamental. The idea is then, as far as possible, to reduce statements involving numerical terms to statements involving only equinumerosity and isomorphism...[Hallett 1984 p.245]

The second detail concerns the treatment of the 'ϵ' relation as a 'tree structure'. In Part II we discussed the manner in which Skolem transformed Zermelo's set theory into a *first-order* theory essentially by taking definite properties to be first-order formulas in a language whose only non-logical symbol was a binary predicate for the 'membership' relation. Thus, in more abstract terms, Skolem may reasonably be construed as having formulated the first-order theory of 'ϵ'. With the postulation of well-foundedness this theory may, quite naturally, be interpreted in terms of 'tree-structure', albeit a rather complicated one. In fact, there has been a continuing theme of working on set theory directly in terms of tree-structure. As early examples we may cite Skolem himself and also Mirimanoff both in connection with well-founded sets. Mirimanoff [1917] was interested in 'ordinary' sets which he described as having finite 'membership chains'.

Perhaps more interestingly, Skolem described what we would now denote as an 'inner model' B' of set theory within "a *domain* B" [my italics] composed of those sets with finite 'ϵ-sequences'. Then starting with a model composed of such sets each of which terminates at the null-set he shows how to form an extended domain by adjoining a *new* element. [Note the analogy with forcing - see Appendix] This is all facilitated by the observation that "with every set M we can associate a corresponding ϵ-tree.." [1922 p. 298] His new model is then built up from augmented trees. It should be stressed that neither Skolem nor

Mirimanoff suggested that sets *were* well-founded or not. They were simply investigating different models of the axioms.

I have used the above examples to underline an algebraic attitude on the part of certain set theorists. However, in general, the use of the tree-structure analysis has not proved particularly important for mainstream set theory. But it does figure in two important developments. First, It has been adapted within descriptive set theory, which chiefly concerns itself with the application of set theoretic methods to the analysis of the continuum. In this discipline, although the notion of a 'tree' is widely employed it is significantly altered. It is defined as a partially-ordered set $\langle T, \leqslant_T \rangle$ such that for any $x \epsilon T$ the set $\{y \epsilon T : y \epsilon x\}$ is well-ordered by $\leqslant_T$. The first sytematic studies of such trees appears in Kurepa 1935 in connection with his work on the Souslin Hypothesis and manifestly diverges from its use in analysing the membership relation. This is not the case with respect to the second development.

This second development is the establishment of a programme begun more or less immediately after the formulation of topos theory. Given topos theory as a generalized set theory it is natural to ask how we may add to it in order to approximate (categorially) classical set theory. In a sense this may be seen as a continuation of Lawvere's 1964 programme of axiomatizing the category of sets. Two of the tasks undertaken ,for example, might be to give categorial accounts of the axioms of extensionality and well-foundedness. More specifically, the programmes' aim, as described by Johnstone is to prove that (elementary) topos theory, with the addition of certain "set-like" axioms, is *logically equivalent* to a certain "weak" version of Zermelo-Fraenkel set theory. [1977 p.303]

The approximating system referred to by Johnstone is Z_ω whose characteristic feature is that it allows only formulas with bounded quantification to figure in the separation schema. This programme was initiated independently by Mitchell and Cole in their respective papers of 1972. Referring to these, Osius informs us

..the construction of the model of set theory is based on a set-theoretical idea which goes back to Specker..[1957]...namely that the membership relation on a set can be characterised by a 'tree' having certain properties. [1974 p.80]

However Osius was not satisfied with their use of trees. He continues with

Since the concept of a tree does not seem to be a "natural" concept in topos theory (although definable there), one might say with due reference to the pioneer work of Cole and Mitchell that their construction of the model appears as a translation of set-theoretical ideas into the language of topos theory, which is not very 'categorical' in a more general sense. [p.80]

Osius claims his own formulation offers "a simpler and more general method to define models of set theory within topos theory". [1974 p.80] One aspect of his method deserves comment here. His method of characterizing the membership relation involves the shift from a 'local' condition to a 'global'. This is is a common feature of topos theory particularly in its guise as a category of sheaves and generally in the application of sheaf-theoretic methods e.g. forcing in a categorial setting. [See Chapter 5] To finish, in the following quotation, Hatcher neatly summarises Osius' strategy

> The basic observation .... is that, though the membership relation is defined globally on the whole universe of sets, any two sets x and y can be considered as elements of an englobing set z, reducing the question of the membership between x and y to that of determining the membership among elements of z. Thus, the global membership question can, in any given instance, be reduced to a "local" membership question concerning elements of a fixed set z.
>
> In particular, if z is transitive, then x and y will also be subsets of z. But any set z is contained as a subset of a transitive set, namely its transitive closure t. We can therefore "imitate" the membership relation of Z_0 if we can give a categorical definition of membership between two monomorphisms with transitive codomain (representing two given elements of a transitive set). [1982 p.299].

For the third detail the focus of attention shifts to the application of set theory. Cantorian set theory grew out of Cantor's work in analysis, specifically problems concerning the representation of functions by trigonometric series; and subsequently point set theory. From its inception, set theory has been applied in the study of the continuum and analysis in general. But from early on in this century it has been increasingly applied to problems in the fast growing area of algebra. In fact, as we discussed above, set theory is inextricably bound up with this growth. In the ensuing, the point is that we also observe that this growth produced a feedback effect on the application of set theory.

Now we have stated that the characteristic feature of the mathematical approach to set theory is its concern with transfinite number theory. Any axiomatization of set theory within this approach must generate such an arithmetic. However, to underline the algebraic view of set theory in its application we point out that there was a marked shift from the use of ordinal arguments or proofs making explicit use of transfinite arithmetic to maximal arguments. The application of maximal arguments is a standard algebraic technique, for example, the use of Zorn's lemma is ubiquitous.

Hausdorff, a pioneer in the application of set theory, is credited to have given the first explicit formulation of a maximal principle in his 1909. However with the development of abstract algebra in the twenties and thirties the use of maximal principles comes into prominence and begins to overshadow heuristics involving ordinal arguments e.g. those based on the well-ordering principle. In fact such principles were seen as essential to the progress of abstract algebra. Perhaps the most famous of these principles is Zorn's Lemma formulated by Zorn in 1935 The use of this principle finally established the supremacy of maximal arguments over ordinal arguments. The basic reason for the switch seems to be a matter of pragmatism. That is, the form of a maximal principle e.g. Zorns Lemma is more directly applicable and suggestive in an algebraic context than ordinal arguments. Furthermore they have the heuristic advantage that they tend to be applicable in a more uniform manner. This is brought out in the following passage on Kuratowski's use of maximal principles in his development of Dedekind's chain theory:

As noted by Kuratowski [1922] the theory of transfinite numbers had found numerous applications in different branches of analysis and of topology, although the actual theorems established scarcely contained any reference to these numbers. Moreover, a number of mathematicians had succeeded in eliminating transfinite numbers from their theories by means of special, *ad hoc*, devices. Kuratowski therefore proposed a simple, general and uniform method which could be applied in all these applications without any reference to transfinite theory...."
[Temple, G. '100 Years of Mathematics' 1981 p.40]

And discussing the impact of Zorns Lemma Moore adds

> Why, then, did maximal principles not attain prominence at an earlier date? Since the deductive strength of such principles was no greater, and no less, than that of the Axiom [of Choice], their use as a substitute for the Well-Ordering Theorem was largely a matter of convenience. Steinitz had applied this theorem liberally in algebra, but by the 1930's some of his successors considered it, together with transfinite ordinals, to be a transcendental device and not properly a part of algebra. Hence there arose in algebra a perceived need to replace both ordinals and the Well-Ordering Theorem by a more algebraic device. Yet since maximal principles had been formulated previously only as peripheral theorems in set theory, algebraists ignorant of them until Zorn illustrated how useful they could be." [1982 p.226-7]

In conclusion it should be noted that initially, and for some time after, these maximal principles were applied as lemmas or alternatives to the well-ordering theorem. They were not at first construed as instances of the well-ordering theorem applied in a different but equivalent form. The logical relations amongst these principles were not investigated. This situation only began to change after Zorn's work. However, although after formulating his principle Zorn claimed that it was equivalent to the axiom of choice he did not provide a proof. This was done by Teichmüller [1939] and independently by Tukey [1940].

TOPOS THEORY, LST AND DEFINITE PROPERTIES.

(i) Definite properties and topos theory.

The notion of 'definite property' has been a central, essential, and continuous theme in set theoretical thinking since Cantor's original explications of the concept of set. The enterprise of axiomatizing and formalizing set theory beginning with Zermelo, through the work of those such as Weyl, Skolem, Fraenkel, von Neumann and right up to the present day essentially involves the explication of definite property. I have discussed definite properties at some length in Part II, in particular, focusing on Skolem's formulation and it is evident, I hope, from that discussion just how crucial this notion has been for the development of the set concept within the mathematical approach. Unfortunately its importance has often been overlooked or at least understated by commentators on set theory, especially with respect to its connection with Cantorianism.

Usually built into the axiom of separation, as such this notion may justifiably be said to constitute the 'overlap' between the mathematical and logical character or content of set theory. Furthermore, as is made clear below, the notion is intimately related to the character of the *internal* logic of set theory. In fact the formulation of the axiom of separation by Zermelo is often referred to as his version of the comprehension principle. Its role was to facilitate the 'fine work' of reconstructing mathematics within set theory - capitalizing on the results in this field already achieved by the logicists, e.g. in Russell's 'Principles of Mathematics' (1903).

In topos theory the Ω-axiom facilitates the provision of an analogue to the Separation Axiom seen as a restricted comprehension principle in the following way. First let us consider the situation in the category of sets as follows. Let X be a set and ϕ a definite property. Then the separation axiom yields the subset of X $\{x: x \in X \ \& \ \phi\}$ which we denote as X_ϕ. Let $\phi: X \to 2$ be the characteristic function of X_ϕ and $\{x:\phi\}: X_\phi \to X$ the inclusion map. For any $y \in X$, $y \in X_\phi$ iff $\phi(y)=1$ and the the following diagram is a pullback

$$\begin{tikzcd} X_\phi \arrow[r, "\{x:\phi\}"] \arrow[d] & X \arrow[d, "\phi"] \\ 1 \arrow[r, "true"] & 2 \end{tikzcd}$$

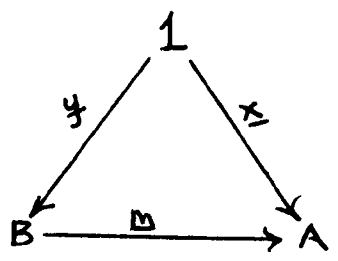
[diagram 9]

Let $\phi: A \to \Omega$ be a generalized characteristic function i.e. an arrow with codomain Ω. (Lawvere calls these 'propositional functions' - see his 1972 p.3). There then exists a monic (the topos correlate of inclusion) $\{x:\phi\}: A_\phi \to A$ such that the following diagram is a pullback

$$\begin{tikzcd} A_\phi \arrow[r, "\{x:\phi\}"] \arrow[d] & A \arrow[d, "\phi"] \\ 1 \arrow[r, "true"] & \Omega \end{tikzcd}$$

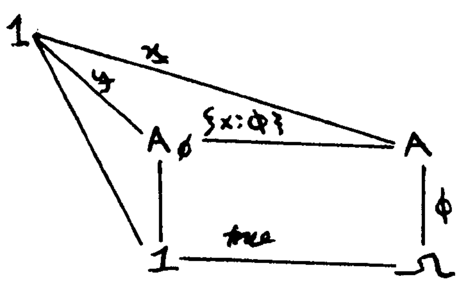
[diagram 10]

Now the following 'membership' relation is defined between elements of an object and monics with that object as codomain. Recall that for an object A, we defined an 'element' of A to be an arrow with domain 1 and codomain A i.e. $\mathbf{x}: 1 \to A$. Let $\mathbf{m}: B \to A$ be an arbitrary monic with codomain A. We define $\mathbf{x}$ to be a member of $\mathbf{m}$ if there exists an element of B i.e. $\mathbf{y}: 1 \to B$ such that the following diagram commutes.



[diagram 11]

Thus $\mathbf{x}=\mathbf{m}^{\circ}\mathbf{y}$. The rationale for this definition becomes clear if we think of $\mathbf{m}$ as an inclusion in the category of sets. Now consider the following diagram which is an extension of the pullback square given above.



[diagram 12]

Now suppose $\mathbf{x} \in \{x:\phi\}$ i.e. $\mathbf{x}=\{x:\phi\}^{\circ}\mathbf{y}$. Since from any object there exists a unique arrow with codomain 1 it follows that $1_1 = 1_A{}^{\circ}\mathbf{y}$. By assumption $true^{\circ}1_A = \phi^{\circ}\{x:\phi\}$ and thus $true^{\circ}1_A{}^{\circ}\mathbf{y} = \phi^{\circ}\{x:\phi\}^{\circ}\mathbf{y}$. So from $\mathbf{x} \in \{x:\phi\}$ it follows that $\phi^{\circ}\mathbf{x} = true$. On the other hand if we postulate that $\phi^{\circ}\mathbf{x} = true$ by the universal property of pullbacks $\mathbf{x} \in \{x:\phi\}$. We have then

$$\mathbf{x} \in \{x:\phi\} \text{ iff } \phi^{\circ}\mathbf{x} = true$$

And, as the notation is designed to suggest, this is the internalization of the restricted comprehension axiom.

It is important to note (particularly in connection in what we have to say about the interpretation of LST) that there is a duality between arrows of the form $\chi: A \to \Omega$ and monics with codomain A. For monic $m: B \to A$ there is (according to the Ω-axiom) a unique arrow with codomain Ω for which it is the pullback. Conversely given $\chi: A \to \Omega$ there is, in a sense, a unique 'subset' of A associated with $X: A \to \Omega$. If we represent that 'subset' by a monic, say, $m: B \to \Omega$ then $\chi: A \to \Omega$ classifies $m: B \to A$. From the category theoretic point of view the subset is unique since it is determined up to isomorphism. But since the emphasis here is on arrows, specifically monics, the notion of a 'subobject' is developed. For an object A a subobject of A is an equivalence class of monics. Two monics $m: B \to A$, $n: C \to A$ are equivalent in case there is an arrow $i: B \to C$ such that $m \cdot i = n$. The collection of subobjects for an object A is denoted Sub(A). Moreover the function carrying objects to subobjects is a set-valued functor. We thus obtain for a topos the following *natural isomorphism*

$$\mathrm{Sub}(-) \cong \mathrm{Hom}(-\Omega) \qquad (\#)$$

Incidentally, Lawvere is quite forthright about the meaning of the Ω-axiom. He states that

The characterizing property of the set Ω,...the truth values of a topos *E*, is that there is a distinguished eternal truth value *true*: $1 \to \Omega$, the inverse image $\{X \mid \phi\}$ of which along $\phi: X \to \Omega$ is of course a subset of X for which

$$x \in \{X \mid \phi\} \text{ iff } \phi \cdot x = true_T, \text{ for any } x: T \to X,$$

but which moreover is such that *any* subset of any X is of the form for a *unique* ϕ. [Lawvere 1976 p.121. N.B. here $x: T \to X$ is to be construed as a 'generalized element' or 'element of X varying along T' as opposed to a 'constant element' i.e. $x: 1 \to X$. The arrow $true_T$ is the composite $true \cdot Id_T : T \to \Omega$]

In the above account of the topos analogue of the separation axiom the role of definite properties has been played by Lawvere's 'propositional functions' . Given an object A and a propositional function on A there is a 'subset' of A induced by that propositional function. I have phrased it deliberately in Lawvere's terminology in order to bring out the resemblence to Zermelo's approach and this resemblence will become more evident as we proceed.

Now the topic of definite properties brings us directly to the question of the relationships between the notions of properties, formulas, and subsets. These relationships have been 'uneasy' at best. The topos-theoretic approach, in its incarnation as LST, being a generalization of set theory is no exception with respect to its concern with definite properties and, as we shall see, within this framework they emerge in a particularly interesting guise, especially in respect of the aforementioned 'uneasy relationship'. More specifically, in the formulation of LST they are an appropriate refinement of Skolem's reformulation of Zermelo's account of definite property and constitute a natural endpoint in that they resolve certain issues relating the relationship between subsets, power sets and formulas engendered by Skolem's version. In this connection a significant role is played by the natural isomorphism (#).

But having acknowledged its logical connections it must be emphasized that the notion of a definite property is nevertheless a manifestation of the mathematical approach to set theory. To begin with there was here no claim that in any respect it was the notion of 'property' *per se* that was being explicated. Nor was there any concern to any significant degree with a general analysis of properties or mathematical properties. Set theory is sometimes described as the 'theory of extensions of arbitrary properties'. But, if anything, this construal relates to the concerns of the logical approach not the mathematical. The different accounts of definite property by Zermelo, Skolem etc. did not spring from philosophical analyses of property as such. Moreover, the various reformulations of Zermelo's notion, variously motivated, are more in the way of being stipulations. Each stipulation underpinning the set theorists particular concerns. (Of course, the essential constraint on these formulations was that they allow the axiom of separation to perform its original quasi-constructive role.)

As a restricted comprehension Zermelo's axiom of separation generated sufficient sets for the quasi-construction of extant mathematics. The point of adding the constraint of 'definiteness' upon the range of his second order quantifiers was the avoidance of the semantic paradoxes. As discussed in part II, although it is commonly held that the notion of definite property originates in the early work of Zermelo in fact Zermelo was adopting a Cantorian idea. If we look at its Cantorian precursor it is arguable that Zermelo built it into his axiom in order to regulate the logic of the relation between putative subsets of a given set and the members of the set - just as the logic of Ω determines the logic of subobjects -except, perhaps, that Zermelo couched his discussion in terms of 'propositional functions'. But at the same time, as we have noted, he was ambiguous with respect to the syntactic versus semantic interpretation. Anyway, we observed that in the 1895 paper Cantor offers the following characterization

By an "aggregate" we are to understand any collection into a whole M of definite and separate objects *m* of our intuition or our thought. [1985 p.85 of Jourdain's 1915 translation]

It is worthwhile quoting again Fraenkel's comments on the qualification 'definite' in Cantor's characterization. He writes

[it]...expresses that, given a set s, it should be intrinsically settled for any possible object x whether x is a member of s or not. Here the addition "intrinsically" stresses that the intention is not to actual decidability with the present (or with any future) resources of experience or science; a definition which intrinsically settles the matter, such as the definition of "transcendental" in the case of the set of all transcendental numbers, is sufficient. To be sure, we thus essentially use the Aristotelian *principle of the excluded middle* which guarantees that for a given object there is no case additional to those of belonging or not belonging to the set in question. [A.Fraenkel, 1968 "Abstract Set Theory" p.10]

So, for example, the 'collection of natural numbers definable in less than nineteen words' is not a Cantorian "aggregate". That is, if we accept Fraenkel's interpretation. But Zermelo in his adoption of this notion for his axiom of separation (built in to bar the semantic paradoxes) had this idea in mind. Given a X-definite "propositional function" P(x) (recall that for Zermelo definiteness is always relative to some set X) and an object $\underline{a}$ of X then either P($\underline{a}$) holds or not and paraphrasing Fraenkel there is no case additional to holding or not holding. In terms of characteristic functions: P(x) is X-definite iff there is a function $\chi: X \to 2$ such that for $\underline{s} \in X$ $\chi(\underline{s})=1$ iff $P(\underline{s})$ holds and $\chi(\underline{s})=0$ iff $P(\underline{s})$ doesn't hold. Note also that a classical characteristic function defined over a given set X generates an "aggregate" in Cantor's sense i.e. $\chi^{-1}[1]$. It is definite by design or construction, so to speak. This comment also carries over to Lawvere's propositional functions which are generalized characteristic functions.

$\chi^{-1}[1]$ is also a subset of X according to the underlying heuristic of Zermelo's Separation axiom, namely: limitation of size. From an extensional point of view $\chi^{-1}[1]$ can be thought of as a definite property over X or perhaps even $\chi: X \to 2$ may be taken as such. This latter, of course, is a 'propositional function' in Lawvere's sense understood in the category of sets and is generalized for an arbitrary topos. Or it might be understood that $\chi^{-1}[1]$ implies the existence of a definite property of which it is the extension. Zermelo's second order formulation of the separation axiom (especially given his equivocation between a semantic and syntactic reading of definite property) might suggest this reading of Zermelo. In that case (#) is the topos correlate. But since Zermelo was not forthcoming as to his philosophy of properties any conjectures on the above must be regarded as tentative.

ZF set theory adopts Skolem's first-order version of the axiom of separation and in particular his notion of definite property. Skolem's characterization is unequivocal - definite properties are to be understood as formulas of the first-order language whose only non-logical symbol (with the possible exception of individual constants) is 'ϵ' . However, a 'gap' is opened up between definite properties in the sense that they are no longer relativized to sets. The notion is no longer local. But if we read (or stipulate) Lawvere's propositional functions as the appropriate correlate of definite properties for a topos then definite properties are, just as in Zermelo's case, localized.

Skolem's statement that his notion of definite property is "completely clear and one that is sufficiently comprehensive to permit us to carry out all ordinary set theoretic proofs" emphasizes the foundational pragmatics of the mathematical approach. But in following Skolem ZF has inherited the following problematic feature, namely: that the relationship between definite properties and the subsets of a given set is left in a somewhat unsatisfactory state. Intuitively, the power set of a given set comprises all the subcollections of that set. But how is the constitution of this power set related to definite properties? For a given definite property and set there is a subcollection of that set correlated with the definite property. However, the relationship of an arbitrary subset or member of the power set of a given set to definite properties is unclear. I am not claiming that this cannot be said of Zermelo's approach. The problem here is that Zermelo is unforthcoming as to the status of propositional functions. Perhaps the issue was only obscured by his second order formulation. What is certain is that there was no general analysis of 'properties' and their relationship to sets within the mathematical approach. For toposes the relationship between definite properties and subsets, that is, the relationship between what we take the appropriate correlates of these, begins to be clarified by (#).

Now whilst LST is conservative in that it essentially retains the ZF (i.e. Skolem's) version of definite properties in the sense that definite properties are taken to be formulas, one of its progressive features is that it goes some way in resolving what amounts to a dichotomy between definite properties and subsets. Moreover, LST goes further in the direction of 'reducing gaps' (e.g. between arbitrary members of a power set and definite properties or, more generally, between theories and models) in the sense that it does justice to the dictum 'the theory is the model'. Or in the words of Lambek and Scott "A topos then does not contain, but actually coincides with its internal language". [1986 p.246]

(ii) Internal languages and LST.

It is in the formulation of LST that topos theory is fully realized as a generalization of set theory. LST arose through the discovery and work on the internal languages yielded naturally by a topos. These languages are generally held to have been discovered independently by Mitchell, Bénabou and Joyal. The first formal account of such a language was given by Mitchell in his 1972.

According to Johnstone we may trace the motivation for their development to the categorial analysis of Cohen's forcing method. He states that

In view of the Lawvere-Tierney proof of the independence of the continuum hypothesis..it became a matter of importance to determine the precise relationship between elementary topos theory and axiomatic set theory. The answer was found independently by J.C.Cole..[1973] and G.Osius [1974].. [1977 p.xv]

The work of Mitchell, Cole and Osius was a continuation of Lawvere's programme of characterizing the category of sets. Cole and Osius worked towards an extension of elementary topos theory approximating ZF, by providing, for example, categorial correlates for the axioms of foundation and replacement. Mitchell, more generally, characterizes the category of sets arising from Boolean-valued models of set-theory. One of the best known results of this programme is the logical equivalence between the theory of well-pointed topoi (i.e. topoi whose skeleton contains at least two objects and if two arrows $f: A \to B$, $g: A \to B$ are such that for every element $x: 1 \to A$ $f \cdot x = g \cdot x$ then $f = g$) is logically equivalent to Z_0. According to Hatcher the theory of well-pointed topoi has identified the 'core' of set theory. However, although these results are interesting in so far as they constitute an investigation into the set theoretic structure that may be 'lifted' from the category of sets into topos it is the more general notion of a topos in its incarnation as LST that is of greater interest. For it is the 'generalized' set concept that proves the most fruitful.

But this 'lifting' did suggest that some of the reasoning and heuristics of topos theory in category theoretic terms might be replaced by the more familiar arguments along set-theoretical lines. This was the use to which the internal languages were put. As Osius puts it

This set theoretical method of investigations [i.e. internal languages] in topos theory has the advantage, that - once the set theory... has been developed to a certain extent - it allows to immediately proceed from a heuristical set theoretical idea or construction to the corresponding result in topos without having to wrestle with lots of diagrams (getting bigger and bigger)." [Osius 1975 p.297]

Hatcher presents the position as follows

Use of the internal languages..for toposes..can replace appeal to principles of functor theory in establishing complicated properties of toposes. Workers in the field of topos theory differ considerably as to their preference for the linguistic approach or the functorial approach to studying toposes. On the one hand, the linguistic approach allows one to reason in a set-like language about toposes and thus to transfer certain thought patterns from set theory to topos theory. There is, however, the drawback that the linguistic approach does not give us much feeling for what is going on since the connection between the reasoning in the formal language and the toposes themselves is made via ..[a].. fairly complicated interpretation function...Thus one finds oneself constantly asking "Now what does this *really* mean?" On

the other hand, the functorial and diagrammatic approach to topos theory has the advantage of allowing one to handle the toposes directly and to "see" schematically through the use of commutative diagrams and functors exactly why and how certain principles work. The heart of set-theoretical reasoning is the abstraction principle by which one simply thinks up the property one needs, writes it down, and and then declares that there is a mathematical object (set) which satisfies the property. With the diagrammatic-functional approach, one must supply the link between concepts and objects since morphisms and functors must be explicitly defined. Functorial reasoning is therefore more explicit or "constructive" and contains more information than does set-theoretical reasoning, but one must pay the price of the extra effort necessary to obtain this extra information.
[1982 pp. 310-11]

It must be stressed, however, that the shift to working with internal languages is not a prescription for eschewing categorial methods in foundations generally or topos theory in particular. The foundational insights and machinery of category theory are still essential. This is because, among other things, it is necessary to analyse and control the mathematical changes brought about by the movement from one 'site of mathematical activity' to another and this is where functors, adjoints play an essential part. Or, as in LST, we might find versions of the language and theory based giving us a better feeling "for what is going on". In short, in adopting LST (or topos theory in general) we retain the set concept as the foundational base, but at the same time it is not a matter of choosing between set theoretical and categorial heuristics and methods but rather developing both in parallel and making the most of what is certainly a very progressive *interaction* for foundations.

At this point we turn to a description of LST and its properties. The account given here is that of the theory formulated by John Bell in his forthcoming [197-]. And, as Bell emphasizes, in the consideration of LST we will be "furnishing the precise sense in which toposes are to be regarded as generalizations of the category of sets".

*

To begin with let us briefly recap on the general structure of a topos. A topos is a category and thus is composed of two basic collections: objects and arrows. The initial heuristic of LST is to construe objects as 'types'. The category exhibits the following additional features: a terminal object 1; finite products of arbitrary objects; an object of truth-values Ω and 'truth' arrow *true*: $1 \to \Omega$ acting together as a subobject classifier; power objects i.e. for each object A an exponential Ω^A. As we have observed the internal logic of a topos is governed by the algebra of truth-values i.e. elements of Ω. In general its structure, as it has turned out, is of a complete Heyting algebra and thus the general internal logic is intuitionistic. This then is the basic raw material for the internal language which we shall refer to as the 'local language'. After this language is constructed the local set theory i.e. the system LST is formulated within it. As in classical set theory the system is built around the primitives '{:}','ϵ' and '=' (respectively: abstraction, membership and equality)

A local language *L* essentially consists of two collections of symbols: type symbols and terms. (Where each term is associated with a given type.) These collections are defined recursively from an initially specified stock of symbols which are as follows:

(S1) $1, \Omega$ (the *unity* type symbol and *truth-value* type symbol respectively.)

(S2) $A, B, C\ldots$ (a collection (possibly empty) of *ground* type symbols.

(S3) $x_A, y_A, z_A, \ldots\ldots$ (a collection of variables associated with each type.)

(S4) # (this will denote the term of type 1)

(S5) f, g, h (a collection (possibly empty) of *function* symbols)

The *type symbols* of *L* are defined to be as follows:

(TS1) $1,\ \Omega$

(TS2) $A, B, C, \ldots$

(TS3) if $A_1, \ldots A_n$ are type symbols then so is $A_1 x \ldots x A_n$. $(n \geqslant 1)$

(TS4) if A is a type symbol, so is PA.

To each function symbol we assign a signature of the form $A \rightarrow B$, where A, B are type symbols. Now the *terms* of L and their associated types are defined to be as follows:

(T1) #

(T2) the variables $x_A, y_A, z_A, ..$ for each type A.

(T3) if f is a function symbol of signature $A \rightarrow B$ and τ is a term of type A, then $f(\tau)$ is a term of type B.

(T4) if $\tau_1, ... \tau_n$ are terms of types $A_1, ... A_n$, then $\langle \tau_1 ... \tau_n \rangle$ is a term of type $A_1 x ... A_n$ $(n \geqslant 2)$

(T5) if τ is a term of type $A_1 x ... x A_n$ $(n \geqslant 2)$ then $(\tau)_i$ $(1 \leqslant i \leqslant n)$ is a term of type A_i.

(T6) if α is a term of type Ω, x_A of type A then $\{x_A : \alpha\}$ is a term of type PA.

(T7) if σ, τ are terms of the same type, then $\sigma = \tau$ is a term of type Ω.

(T8) if σ, τ are terms of types A, PA respectively, then $\sigma \in \tau$ is a term of type Ω.

(Terms of type Ω are called formulas. And note that each formula α gives rise to an abstract $\{x_A : \alpha\}$. For convenience we write variables of type Ω as $\omega, \omega_1, ..$etc and the syntactic variables over formulas as α, β, γ..etc. Where the context allows typical ambiguity is employed. Also substitution, freedom and bondage are handled in the standard manner.)

We have not so far, as is usual for example in classical first-order systems explicitly provided 'logical' symbols. However it is not too difficult to appreciate that the above language has its genesis in the internal picture of a topos and we have also noted how the logic is also generated internally. Thus it is not too surprising that L already contains the machinery to express logical operations. In fact these are defined as follows:

(L1) $\alpha \leftrightarrow \beta$ for $\alpha=\beta$

(L1) *true* for $\#=\#$

(L3) $\alpha \& \beta$ for $\langle \alpha, \beta \rangle = \langle \mathit{true}, \mathit{true} \rangle$

(L4) $\alpha \rightarrow \beta$ for $(\alpha \& \beta) \leftrightarrow \alpha$

(L5) $\Pi x \alpha$ for $\{x: \mathit{true}\}$

(L6) *false* for $\Pi \omega . \omega$

(L7) $-\alpha$ for $\alpha \rightarrow \mathit{false}$

(L8) $\alpha \vee \beta$ for $\Pi \omega [((\alpha \rightarrow \omega) \& (\beta \rightarrow \omega)) \rightarrow \omega]$ (ω not occuring free in α or β)

(L9) $\Sigma x \alpha$ for $\Pi \omega [\Pi x (\alpha \rightarrow \omega) \rightarrow \omega]$ (ω as in L8)

A property of the systems of local set theory is that these logical operations as just defined obey the rules of intuitionistic logic. We now present the basic axioms of a local set theory. The system is given as a sequent calculus.

<u>Tautology</u> $\alpha : \alpha$

<u>Unity</u> $: x_1 = \#$

Equality $:\tau=\tau$

$x=y,\ \alpha(z/x):\alpha(z/x)$ (with x, y free for z in α)

Products $:\ (\langle x_1 \ldots x_n\rangle)_i = x_i$

$:\ x=\langle(x_1 \ldots x_n)\rangle$

Comprehension $:x\in\{x:\alpha\}\leftrightarrow\alpha$

Finally the following are the inference rules

Thinning
$$\frac{\Gamma:\alpha}{\beta,\Gamma:\alpha}$$

Cut
$$\frac{\Gamma:\alpha \quad \alpha,\Gamma:\beta}{\Gamma:\beta}$$
(any variable free in α is free in Γ or β

Substitution
$$\frac{\Gamma:\alpha}{\Gamma(x/\tau):\alpha(x/\tau)}$$
(τ free for x in Γ,α)

Extensionality
$$\frac{\Gamma:x\in\sigma\leftrightarrow x\in\tau}{\Gamma:\sigma=\tau}$$
(x not free in Γ,σ,τ)

Equivalence
$$\frac{\alpha,\Gamma:\beta \quad \beta,\Gamma:\alpha}{\Gamma:\alpha\leftrightarrow\beta}$$

The notion of proof is the standard one for a sequent calculus. If the sequent $\Gamma:\alpha$ is derivable from a set of sequents S we write $\Gamma\vdash_S\alpha$. If $\Gamma\vdash\alpha$, i.e. $\Gamma\vdash_\emptyset\alpha$ we say $\Gamma:\alpha$ is a *valid* sequent.

A ***Local Set Theory*** in L is defined to be a collection S of sequents which is closed under derivability. Given a set S of sequents using the prescription $(\Gamma:\alpha)\epsilon S*$ iff $\Gamma\vdash_S\alpha$ we generate the local set theory S* for which S are said to be a set of axioms for S*. (Not to be confused with the basic axioms.) The local set theory in L generated by the empty set of axioms is called 'pure' local set theory and denoted by L. The local language with no ground types or function symbols is called the 'pure' local language and is denoted by L_0. The pure local set theory in L_0 is denoted by L_0.

*

The first important property of an LST S is that it determines a category of generalized sets C(S) i.e. C(S) is a topos. CS) is constructed from the syntax of S. In fact, the heuristic for deriving C(S) from S closely resembles the manner in which given a classical set theory we may derive a topos. (On the semantic level sets become objects and functions arrows. Thus when considering the theory *qua* syntactic object we look for their term correlates. The process is very much like Henkin's construction of the canonical model for a first order theory.) To make this clearer let us first look at some of the implicit set theory inherent in a local language.

Consider formulas of the form $\tau\in\sigma$ where τ is of some type A. Then the type of σ is PA. Only terms associated with a power type 'have elements'. $\{x_A : x_A\in\sigma\}$ is also of type PA. According to the comprehension axiom we have $x_A\in\{x_A : x_A\in\sigma\}\leftrightarrow x_A\in\sigma$. Whence by extensionality we may infer that $\{x_A : x_A\in\sigma\}=\sigma$. In other words, not only is it the case that only terms associated with power types have members they are also determined by their members in the sense just described. Clearly, if we are looking for 'set-like' terms within our local language terms associated with power types are the natural candidates. Thus, we make the following definition. The 'set-like' terms of a local language *L* are the terms associated with power types. A closed set-like term will be called an '*L*-set' (or 'set' where the relevant language is clear).

Having identified the sets within S we can emphasize their set theoretical nature by demonstrating that they in effect form a set theory within S in the sense that they satisfy further familiar properties. First of all we show some examples of how we may define the usual operations and relations on them. In the ensuing we denote sets by upper case letters A, B, C, ... and to simplify matters we use the following abbreviations:

$$\Pi x\epsilon A.\alpha \quad \text{for } \Pi x(x\epsilon A\rightarrow\alpha)$$

$$\Sigma x\epsilon A.\alpha \quad \text{for } \Sigma x(x\epsilon A\&\alpha)$$

$$\Sigma! x\epsilon A.\alpha \quad \text{for } \Sigma! x(x\epsilon A\&\alpha)$$

$$\{x\epsilon A:\alpha\} \quad \text{for } \{x: x\epsilon A\&\alpha\}$$

We make the following definitions:

$A\subseteq B$ for $\Pi x\epsilon A.\, x\epsilon B$

$A\cap B$ for $\{x: x\epsilon A\&x\epsilon B\}$

U_A or $\mathbf{A}$ for $\{x_A: true\}$

$\emptyset_A$ or $\emptyset$ for $\{x_A: false\}$

PA for $\{u: u\subseteq A\}$

$\cap C$ for $\{x: \Pi y\epsilon C.\, x\epsilon y\}$

$\{\tau\}$ for $\{x: x=\tau\}$ (x not free in τ)

$\{\sigma,\tau\}$ for $\{x: x=\sigma\vee x=\tau\}$ (x not free in σ,τ)

$\{\tau:\alpha\}$ for $\{z: \Sigma x_1 \ldots . \Sigma x_n((z=\tau)\&\alpha)\}$ (z not free in τ)

AxB for $\{\langle x,y\rangle: x\epsilon A \;\&\; y\epsilon B\}$

A^B for $\{z: z\subseteq BxA \;\&\; \Pi x\epsilon B\Sigma! y\epsilon A(\langle x,y\rangle\epsilon z)\}$

Now we can now prove the following:

$\vdash A=B \leftrightarrow \Pi x(x\in A \leftrightarrow x\in B)$ (Axiom of extensionality)

$\vdash A \leqslant A$

$\vdash (A \leqslant B \ \& \ B \leqslant A) \rightarrow A=B$

$\vdash (A \leqslant B \ \& \ B \leqslant C) \rightarrow A \leqslant C$

$\vdash (C \leqslant (A \Delta B)) \leftrightarrow ((C \leqslant A) \ \& \ (C \leqslant B))$

$\vdash ((A \cup B) \leqslant C) \leftrightarrow ((A \leqslant C) \ \& \ (B \leqslant C))$ (Axiom of binary unions)

$\vdash x_A \in U_A$

$\vdash -(x_A \in \emptyset_A)$ (Axiom of the empty set)

$\vdash A \in PB \leftrightarrow A \leqslant B$ (Axiom of power sets)

$\vdash (A \leqslant \Delta C) \leftrightarrow (\Pi z \in C(A \leqslant z))$

$\vdash (C \leqslant A) \leftrightarrow \Pi z \in C(z \leqslant A)$

$\vdash x \in \{y\} \leftrightarrow x=y$

$\vdash \alpha \rightarrow \tau \in \{\tau : \alpha\}$

Conjoined with the comprehension axiom the propositions above constitute the the essentials or 'core axioms' of the local set theory in *L*. Why is this set theory 'local'? John Bell explains

The set theory is *local* because some of the set-theoretic operations, e.g. intersection and union, may only be performed on sets of the same type, i.e. "locally". Moreover, variables are constrained to range over given types - locally - in contrast with the situation in classical set theory where they are permitted to range globally over a putative universe of discourse. [Chapter 3 p.28 forthcoming 197-. In connection with the requirement of restricting global quantification in a theory of sets see the discussion in Mayberry 1977. Notice also that we have in this set theory a 'universal' U_A set for each type A.]

According to the heuristic we are are working with sets will constitute the objects of the category to be constructed and functions (which in set theory are, of course, particular kinds of sets) are the arrows. Thus we choose those S-sets which are functions. That is those sets F such that $\vdash_S F \in B^A$. The topos C(S) is then essentially constructed from S-sets and S-functions. I say 'essentially' because we have to make two amendments following from the following considerations. First of all the 'sets' are syntactic objects and thus, as we have understood them, they are *intensional*. Thus we will in general have many S-sets which according to the theory are the same set i.e. A,B such that $\vdash_S A=B$. But whilst it is harmless that they are different *qua* terms they obviously cannot be different *qua* sets (objects) if they are extensionally identical. Hence the objects of the category are taken to be equivalence classes of S-sets under the relation $\approx_S$ defined by $A \approx_S B$ iff $\vdash_S A=B$.

Second, as was observed in the account of category theory, arrows are assigned a specific domain and codamain. This is in contrast to a function in set theory whose codomain is non-specific. That was the reason that arrows are understood as ordered triples. For the same reason we define an S-arrow to be a triple of S-sets $\langle A,F,B\rangle$ where $\vdash_S F \in B^A$. Having made these provisions it can be shown that this collection of objects and arrows form a category C(S). Moreover, just as in the case of set theory C(S) satisfies the axioms of elementary topos theory. Such a topos is called a *Linguistic topos*. [Strictly speaking the term denotes the topos generated from S-sets via the canonical construction of Bell 197-]

Toposes may be used as the basis of a semantics for local languages. Let E be a topos. An interpretation I of L in E is simply an assignment of types to objects i.e. $I(A)=A_I$ satisfying:

$$(A_1 x \ldots A_n)_I = (A_1)_I x \ldots x (A_n)_I$$

$$(PA)_I = P(A_I)$$

$$1_I = 1$$

$$\Omega_I = \Omega$$

Together with an assignment to each function symbol f with signature $A \to B$ of an arrow $f_I : A_I \to B_I$.

The next step is to provide an extension of I to all terms. The basic idea is that these terms are interpreted as arrows using the following prescription. Let τ be a term of type B and $x_1 \ldots x_n$ distinct variables of types $A_1 \ldots . A_n$ including all the variables of τ. Then an interpretion of τ relative to the sequence $x_1 \ldots x_n$ is an arrow

$$|\tau| : (A_I)_1 x \ldots \ldots x (A_I)_n \to B$$

The specific arrow for each term τ is then defined recursively. We forego the details, but for example $|\#|$ ralative to a sequence $x_1 \ldots x_n$ is the unique arrow $(A_I)_1 x \ldots (A_I)_n \to 1$. Note that if τ is a closed term of a type B then $|\tau|$ relative to the empty sequence is an arrow with domain 1 thus $|\tau|$ is an element of B_I. In the case τ is a set $\{y:\alpha\}$ of type PC then $|\{y:\alpha\}|$ is an element of PC_I which in turn corresponds to a subobject of C_I. But what of definite properties, say formulas with one free variable ? Let α be such a formula with free variable x_C. Then $|\alpha|$ relative to C is an arrow $C_I \to \Omega$ a 'propositional function'. And this classifies $|\{x_C : \alpha\}|$.

We next turn to the question of truth and validity of a formula in our local language relative to a given topos and interpretation. Let α be a sentence i.e. a closed term of type Ω. The interpretation of α is an arrow $|\alpha| : 1 \to \Omega$. Under what conditions do we want to say that α is true in the topos i.e. $\models \alpha$? Recall that in our definition of logical terms the truth value 'true' was defined by the formula $*=*$. Suppose we take α to be this formula. Then it computes as the arrow *true*: $1 \to \Omega$. Clearly we want $*=*$ to hold. Construed as an element of Ω, which is a complete Heyting algebra, it is the maximal truth value. For example, if Ω is the collection of open sets of a topological space then the maximal truth value is the whole space. So if $|\alpha|$ computes as $|*=*|$, being 'as true as possible', then at least $\models \alpha$ should be the case. Furthermore, following the general pattern of intuitionistic semantics (and since it is shown that LST yields the theorems of intuitionist logic) the definition of validity should give α as true iff it takes the maximal truth value in the topos. That is, we should end up with $\models \alpha$ iff $|\alpha|$ is the arrow *true*.

We also require that the axioms, given as sequents are valid. So for example, since $\alpha : \alpha$ is an axiom then our definition of validity should yield $\alpha \models \alpha$. Furthermore, since it is truth value that counts we should be able to replace an occurrence of α by any β where $|\alpha| = |\beta|$. That is, if $|\alpha| = |\beta|$ and $|\gamma| = |\delta|$, then if we have $\alpha \models \beta$ we should also have $\gamma \models \delta$.

Since thinning is a rule of inference validity should be maintained if we add sentences to the left hand side of the sequent. For any sentence α, $|\alpha|:1\to\Omega$ uniquely determines a subobject of 1. If we compute $|\alpha\&\beta|:1\to\Omega$ then this is, the infimum of $\{|\alpha|,|\beta|\}$ in the algebra of subobjects of 1. So, if we have $\alpha|=\beta$ we should also have $\alpha\&\gamma|=\beta$. Putting all this together, if $|\alpha|\leqslant|\beta|$ in the algebra of subobjects of 1 then $\alpha|=\beta$ should hold. The above considerations supply a partial heuristic for the following definition of validity.

First, if Γ is a finite set of formulas $\{\alpha_1,\ldots\alpha_m\}$ we write: $|\Gamma|$ for the characteristic arrow of $\inf\{|\alpha_1|,\ldots|\alpha_m|\}$ if $m\neq 0$ or $true:1\to\Omega$ if $m=0$. $\Gamma:\beta$ is said to be valid valid, written $\Gamma|=\beta$, if $|\Gamma|\leqslant|\beta|$, (relative to the sequence of free variables of $\{\Gamma,\beta\}$. Indeed, for a sentence α it turns out that $|=\alpha$ iff $|\alpha|=true$. Relative to this semantics it can be proved that the axioms and rules of inference of a local language are valid under any interpretation in any topos. In fact, we can obtain both the soundness of LST and its completeness with respect to this semantics.

The next property establishes that LST and topos theory are two aspects of the same concept of set. We have seen that from a local set theory S we can construct a topos, the canonical construction yields a 'linguistic topos' C(S). Moreover, this topos is a model for the local set theory. Now given a topos E we can construct a local language L(E) from its objects and arrows. With the exception of the terminal object and object of truth values which are taken as 1 and Ω respectively the objects of E make up the ground type symbols. The product types and power types are defined recursively in the obvious way starting from the products and power objects of ground types. The arrows constitute the function symbols which as usual are triples $\langle f,A,B\rangle$ for arrow $f:A\rightarrow B$. Given $L(E)$ we can induce the 'natural interpretation' by interpreting a ground type symbol and function symbol as the objects and arrows from which they are derived.

The local set theory $Th(E)$ is defined as the theory in $L(E)$ whose axioms are all sequents $\Gamma:\alpha$ such that under the natural interpretation $\Gamma\models\alpha$ holds in E. Since LST is sound it follows that

$$\Gamma\vdash_{Th(E)}\alpha \quad \text{iff} \quad \Gamma\models_{E}\alpha$$

Since $Th(E)$ is a local set theory we can construct the linguistic topos $C(Th(E))$. What is the relationship between this linguistic topos and the topos E we started with? They are the same topos! Formally, we can prove the 'Equivalence Theorem':

$$E\simeq C(Th(E))$$

And from the point of view of category theory they are identical.

Now starting with a local set theory S we may construct the following progression:

$$S \rightarrow C(S) \rightarrow Th(C(S)) \rightarrow C(Th(C(S))) \rightarrow \ldots$$

By the equivalence theorem we have

$$\Gamma \vdash_{Th(C(S))} \alpha \text{ iff } \Gamma \models_{C(S)} \alpha$$

Furthermore, for any term of type Ω in the language of the theory S we have

$$\Gamma \vdash_{Th(C(S))} \alpha \text{ iff } \Gamma \vdash_{S} \alpha$$

Thus the toposes constructed in this progression are stable in the sense that from the point of view of category theory they are identical. And the succession of theories are conservative with respect to provability.

Finally, a remark on the relationship between definite properties and subobjects. We know that a formula (definite property) of L(E) is interpreted as an arrow with codomain Ω, ie. as a 'propositional function' and hence is uniquely associated with some subobject in E. But we can go further in our account of the relationship in question. Given an arbitrary subobject in E its characteristic function generates a formula in L(E) with which the original subobject is uniquely associated.

*

We have observed the set theory inherent within a LST. By appeal to the equivalence theorem, a topos, construed as a site for mathematical activity is seen to embody the set theoretical constructions of LST. In short, the foundations of the mathematical activity within a topos is set theoretical. This last particular underwrites my contention that the shift from ZF to topos theoretic foundations is not a shift away from set theory. *LST is set theory.*

But at the same time we have made use of the category theoretic viewpoint. For example, the 'equivalence' of the equivalence theorem is equivalence of categories. Here there is no tension between set theory and category theory. Both have their appropriate role. In fact, the foundational importance and input of category theory was discussed in Chapter 2 against the background of the increasing emphasis on structure in the evolution of modern mathematics. But as stated, these structures are set theoretical realizations. The job of category theory is to generate the general concepts and heuristics for dealing with structures and their relations. This does not militate against the view that mathematics takes place within a set theoretical universe.

Since mathematics is based within these sites it is a vital foundational task to analyse the relationships between sites and the process of moving from one to another. The best clue we have in this connection is Cohen's forcing technique. One of the insights that categorial concepts afford us is as to the nature of this process.

This surely constitutes foundational progress. It is to the topic of the topos explication of forcing to which we now turn.

THE TOPOS EXPLICATION OF FORCING.

(i) Forcing or/and the continuum hypothesis.

In 1963 Cohen obtained a proof of the independence of the continuum hypothesis from ZF set theory. However, the essential foundational significance of Cohen's result does not emerge from the fact that this *particular* independence result is proved or in fact that any *particular* independence result is proved. The foundational impact of Cohen's result arises out of the forcing technique. Broadly speaking. Cohen's method yields a uniform method of constructing new models of set theory (with certain given desired properties) from a ground model. [For a general overview of the method see Appendix]. The classical universe of sets or models provide 'sites' within which mathematical activity takes place. The application of Cohen's method across the spectrum of mathematical disciplines and the proliferation of independence results has not only forcefully underlined that, as Reyes puts it "..that by 'embedding' mathematical objects in the universe V of sets, these objects inherit the features of the surrounding universe.." [Reyes 1981 p.236] but also the immense richness or variation in this inheritance.

Thus a fundamental foundational task is the analysis and control of this inheritance. Clearly, as matters stand at present, the key here is an understanding of forcing. This is achieved by the explication of forcing in the topos-theoretic framework. Moreover, this analysis shows that in fact the appropriate sites of mathematical activity, and which are implicit i.e. present but invisible, in the forcing method of Cohen are not the classical set theoretic universes. This explication is the main subject of the chapter. But before proceeding with this I want to make a few additional remarks on some of the statements in the paragraph above.

To begin with - on the importance of the particular result that the continuum hypothesis is independent of ZF. This importance certainly does not derive from the fact that it is an example of a shortfall, as such, in the axioms of set theory. Gödel's incompleteness results in 1931, over thirty years previous, informed us that a theory such as ZF not only was incomplete but was not strong enough to prove its own consistency. (Löwenheim-Skolem had also brought out limitations of first-order systems. These limitative results were sufficient for Skolem to occasionally adopt a relativist position.)

However, Cantor had devised his set theory or rather theory of transfinite arithmetic as a 'measure' of mathematical objects and undeniably the continuum has been perhaps the most important object of mathematical investigations - so at least this object must be measured by the theory. Certainly Cantor worked on the problem of the continuum hypothesis throughout his career and Hilbert cited it at the top of his list of the twenty three most important mathematical problems for the new century. So historically it was placed at centre stage of matters set theoretical and consequently the proof of its independence accrued much attention.

The situation engendered by the proof of its independence may to some extent be likened to the Paris-Harrington result in connection with formalized arithmetic. It was known that Peano Arithmetic is incomplete but it may be thought that we may partition true arithmetical statements into those that are "reasonably natural theorem[s] of finitary combinatorics" [Paris/Harrington 1977] or "strictly mathematical statements about natural numbers" [ibid] and those that are metamathematical pathologies with respect to arithmetic proper with the idea that Gödel's result doesn't apply to the former. But the work of Paris-Harrington has demonstrated that this programme is not viable.

Now by analogy, although the incompleteness of ZF and all the other shortcomings of ZF by virtue of it being a first order system were accepted, as long as 'no reasonably natural or strictly mathematical statements about sets' are independent then we need not have any foundational qualms. The notion of 'reasonably natural' or 'strictly mathematical' is, if not for arithmetic then certainly for set theory, obscure. However, the feeling is that at least it should be read to include the continuum hypothesis.

But at the same time the following points should be made that point away from the view that the foundational impact resides in the proof of the independence of the continuum hypothesis. Or if you like, perhaps this particular proof is foundationally important but having taken the fact of its independence on board it turns out that it is the message of the forcing technique itself that is of greater moment. In any case, first, it had already been thought that the continuum hypothesis *would* prove to be independent. For example, Skolem in a prophetic footnote to in his 1922 wrote

Since Zermelo's axioms do not uniquely determine the domain B, it is very improbable that all cardinality problems are decidable by means of these axioms. For example, it is quite probable that what is called the continuum problem, namely, the question whether $2^{aleph(0)}$ is greater than or equal to aleph(1), is not solvable at all on this basis; nothing need be decided about it. The situation may be exactly the same as in the following case: an unspecified commutative field is given, and we ask whether it contains an element x such that $x^2=2$. This is just not determined, since the domain is not unique.
[p.299. Incidentally, note the comparison of set theory with an algebraic theory. I believe this underlines the algebraic view of the those set theorists within the mathematical approach. Also note that the domain is not fixed i.e. B is a parameter.]

More recently, Gödel writing in his 1947, is of the opinion that the continuum hypothesis could not be proven from the axioms of ZF. He gives two reasons. First "the fact that there are two quite differently defined classes of objects both of which satisfy all axioms of set theory" [p.478] i.e. the constructible sets and the other "of the sets in the sense of arbitrary multitudes". The continuum hypothesis predicts the result of counting the subsets of ω, but the notion of set yielded by the axioms is not clear enough to inform us as to which sets are to be counted. Second, he points to facts "not known in Cantor's time" which constitute "highly implausible consequences of the continuum hypothesis". Thus having given a proof of the consistency of the hypothesis with ZF he doubts that it is other than independent.

As a topic internal to set theory the continuum hypothesis has had, and with a certain justification, a high profile. And as we have stated the continuum itself is a structure of prime mathematical importance. But the fact is that the continuum hypothesis has had very little effect if any throughout the greater part of mathematics.

(On a practical note we might add that not only has there been a flood of independence results in group theory, topology, etc but many open problems in these various disciplines that have taxed the time and ingenuity of researchers have been proved to be independent i.e. insoluble in the appropriate sense. Conversely a given independence result saves any researcher embarking upon a fruitless enterprise. Hence it is arguable that any insight into the nature of forcing, at least in this connection, constitutes foundational progress.)

In his forward to Bell's 1977 Scott writes that

> Cohen's achievement lies in being able to *expand* models (countable, standard models) by adding new sets in a very economical fashion: they more or less have only the properties they are *forced* to have...I knew almost all the set-theoreticians of the day, and I think I can say that no one could have guessed that the proof would have gone in just this way....And moreover his method was very flexible in introducing lots and lots of models - indeed, too many models. [p.xiii]

We have already mentioned that an analysis of Cohen's forcing method brings to light the implicit presence of sites of mathematical activity. These come to light as intermediaries between Cohen's ground models and their adjuncts. The nature of these will be made clear in the discussion on Lawvere's notion of 'variable set' below. Thus Cohen's forcing not only yields a key to the passage between one site and another but also indicates the presence of a more extended and richer source of sites. That is, the classical sites are embedded and form part of a denser array of sites. In short, we are not seeing the whole picture. It is here that the explication of forcing in the topos theoretic setting sheds its light. Forcing turns out to be a special case of a natural operation on toposes and furthermore there is a significant increase in our understanding and management of the relations between the sites of mathematical activity as well as the means of smoothly moving between them. This understanding and insight is not at all evident from the entrenched classical point of view. This last consideration reinforces the brand of relativism inherent in the shift to LST. Mostowski's variety, for example, although it stems from Cohen's independence results yields no further insight into their significance beyond the adoption of a bare relativist standpoint.

In the remainder of this chapter the above will be made more concrete by a presentation of the topos theoretic explication of forcing. This is presented in three sections: (ii.a) Grothendieck's generalization of the classical notion of Sheaf; (ii.b) Lawvere and Tierney's generalization of the notion of sheaf to an an arbitrary topos; (ii.c) Forcing and Lawvere's notion of 'variable set.

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(ii.a) Grothendieck's generalization of the classical notion of Sheaf.

The key to the topos theoretic account of forcing is to be found in the concept of a sheaf. The overall program here is to generalize the classical notion of a sheaf over a topological space. Recall that a sheaf is an object in the functor category in $S^{O(X)}$, where O(X) is the set of open sets of the topological space X construed as a category with arrows corresponding to reverse inclusions. The generalization carried out by Grothendieck involves replacing the category O(X) by an arbitrary small category C. This essentially requires formulating the appropriate analogues of "presheaf" and "covering family". Now I have stated that generalization is an important and frequently used conceptual tool of mathematics and in particular algebra. The generalization of 'sheaf' is a particularly interesting example. As will become apparent, the methodology employed is that of analysing the classical case, more specifically finding appropriate equivalent characterizations of the classical notions, or at least key characteristics, which may be more naturally carried over into the general setting. Before embarking on a more detailed exposition let us first sketch in some of the historical background and motivation of the work of Grothendieck and others on this program.

The history of sheaf theory may be said to have its starting point in Oflag XVII, a prisoner-of-war camp. It was there that Leray delivered a course of lectures which formed the subject matter of his 1945 paper. This paper is generally held to have inaugurated sheaf theory. But as Gray puts it

> Sheaf theory, not really being a subject, cannot properly be said to have a history. Rather, it is an octopus spreading itself throughout everyone else's history. Of course, "everyone" is an exaggeration since sheaf theory is a part of geometry; namely, that part concerned with the passage from local to global properties. [1979 p.1]

This transition from the local to the global turns out to be the key to the connection between the Cohen's forcing method and sheaf theory. Loosely speaking, Cohen's forcing conditions carry bits of local information, for example, about a 'new' subset of the natural numbers, which can be 'glued' together to form that subset in an adjunction of the ground model. [See Appendix].

In the 1950's sheaf theory was developed by Cartan, Weil, Serre, Godement, Grothendieck and others. It rapidly became a tool in disciplines such as algebraic topology, complex analysis, analytic geometry, differential geometry, the theory of differential equations and, most significantly in connection with our present concerns algebraic geometry. The central figures in this last discipline were Grothendieck and other members of his "Seminaire de Géometrie Algébrique du Bois Marie" which included Artin, Giraud, and Verdier.

According to Gray one of the central problems addressed by Grothendieck's school was the Weil conjectures. [See Gray 1979 p.39] These conjectures are the generalization of the well-known Riemann hypothesis for mod-p number systems. (These were eventually proved by Deligne in 1974.) An essential notion developed in this field was that of a 'scheme' This development , as Gray states, was due "essentially to Grothendieck who, after abortive attempts by Chevalley and others, found the right generalization of varieties - that of schemes." Johnstone now takes up the story

However.....it was soon discovered that the topological notion of sheaf was not entirely adequate, in that the only topology available on an abstract algebraic variety or scheme, the Zariski topology, did not have "enough open sets" to provide a good geometric notion of localization. ... A.Grothendieck observed that to replace "Zariski-open inclusion" by "étale morphism" was a step in the right direction; but unfortunately the schemes which are étale over a given scheme do not in general form a partially ordered set. It was thus necessary to invent the notion of "Grothendieck topology" on an arbitrary category, and the generalized notion of sheaf for such a topology, in order to provide a framework for the development of étale cohomology. [1977 p. xii]

As we have seen the more general notion that was developed was that of a Grothendieck topos which, facilitated by Giraud's characterization in terms of exactness properties, led to the Lawvere-Tierney theory of elementary topoi. We now turn to some of the details. As Bell points out:

...it is clear from the definition that the notion of a sheaf on a topological space depends only on the lattice of open subsets of the space, and in no way on its "points". This suggests the possibility of extending this notion to categories more general than lattices of open sets. We shall see not only that this possibility can be realized for an arbitrary small category C, but that the resulting theory is an important and illuminating generalization of the classical case. [1982 p.326]

In generalizing the notion of a sheaf in a category of the form $S^{O(X)}$ to one where we replace O(X) by C i.e. to formulate the notion of a sheaf over a small category C, we need a more general notion of a topology and, as stated above, in particular we need the appropriate general notions of a "presheaf" and "covering family". That is: a

Grothendieck topology on a category is a generalization of the concept of all open covers of all open sets in a topological space. Then a sheaf is the appropriate class of presheaves over this topology. The generalization of "presheaf" is more or less immediate. That is, we again take a presheaf to be an object in the functor category $S^{C(op)}$ - *the category of presheaves on* C. F is a subpresheaf of G if for all C-objects X $F(X) \leq G(X)$ and for C-arrow $f: X \to Y$ $F(f) = G(f) \uparrow F(Y)$.

A class of presheaves known as the "representable presheaves" are of special importance in facilitating the extended notion of covering family. Let X be a C-object. Then a representable presheaf is a functor h_X where $h_X(Y) = \hom(Y, X)$ and for C-arrow $f: Y \to Z$ we obtain the function $h_X(f): h_X(Z) \to h_X(Y)$ defined by $h_X(f)[g] = g \cdot f$ for $g \in h_X(Z)$.

On analysing the classical notion of a covering family it was found that their properties could be expressed in terms of sieves. Let $U \in O(X)$ a sieve on U is a family R of subsets of U satisfying: i) $R \leq O(X)$; ii) if $W \in O(X)$ and $W \leq V \in R$ then $W \in R$. The following observation allows us to analyse covering families in terms of sieves. Let $Q \leq O(X)$ be a family of subsets of U. Q generates the sieve $Q' = (V \in O(X): V \leq UQ)$. Clearly, Q covers U iff Q' does. Let J(U) be the set of a "covering sieves" R on U, where $U \in O(X)$. For a sieve R, $V \leq U$, we define $V * R = (W: W \in R$ and $W \leq V)$, i.e the restriction of R to V.

Grothendieck took the following conditions to be "*characteristic* of the notion of covering sieve for sheaf theoretic purposes because, as we shall see, they generalize easily to an arbitrary small category C in such a way as to enable the notion of sheaf to be naturally carried over to C." The conditions are; (i) the '**maximal**' sieve is a covering sieve; (ii) the restriction of a covering sieve to a smaller open set is a covering sieve; (iii) if the restriction of a sieve S to each member of a covering sieve R is a covering sieve, then S is a covering sieve. More formally:

(C1) $\{V \in O(X) : V \leq U\} \in J(U)$

(C2) if $R \in J(U)$ and $V \in O(X)$, then $V^*R \in J(V)$.

(C3) if $R \in J(U)$ and S is a sieve on U such that for each $V \in R$ we have $V^*S \in J(V)$, then $S \in J(U)$.

At this point, then, the task in hand is to form for an arbitrary C-object U, the notion of a *sieve* on U. For the collection of open sets O(X) on a topological space X a sieve is defined in terms of inclusions. But for O(X) construed as a category O(X)-arrows correspond to inclusion. So we may by analogy with O(X)-arrows define a sieve for C-arrows, as follows: for C-object U, a sieve on U is a family R of C-arrows with codomain U, such that if $(\alpha: V \to U) \in R$, then given any $(\beta: W \to V)$, $(\alpha \circ \beta: W \to U) \in R$. From this notion of sieve for an arbitrary category C, employing C-arrows in like manner to O(X)-arrows (i.e. inclusions), the notion of a covering sieve in C is realized by directly translating the conditions (C1)-(C3). More specifically, via the formulation of a 'Grothendieck topology'. This is defined as a set-valued function J with domain all C-objects. J assigns to each C-object U a family J(U) of sieves on U, called J-covering sieves, such that the following (direct translations of (C1)-(C3)) are satisfied:

(G1) for any U, $\{\alpha: \text{codomain}(\alpha)=U\} \in J(U)$

(G2) if $R \in J(U)$ and $f: V \to U$ is any arrow, then the sieve

$f^*R = \{\alpha: W \to V: f \cdot \alpha \in R\} \in J(V)$

(G3) if $R \in J(U)$ and S is a sieve on U s.t. for each $f: V \to U$ in R

we have $f^*S \in J(V)$ then $S \in J(U)$.

A small category equipped with a Grothendieck topology is called a 'site' and is usually denoted (C,J). Grothendieck construed these sites as "generalized topological spaces".

Thus, equipped with the notions *presheaf* and *covering sieve* for an arbitrary category C we proceed to investigate how we may put these together to arrive at a *sheaf* over a generalized topological space. As I mentioned above, the representable presheaves are particularly important - the reason for this is essentially because sieves (which have been our way of working with covering families), in the classical case, may be identified with subpresheaves of representable presheaves - and in fact this identification holds over for an arbitrary category.

First let us consider the classical case. Let R be a sieve on $U \in O(X)$. We can identify R with the presheaf $R(-): O(X)^{op} \to S$ defined by

$$R(V) = \begin{cases} 1 & \text{if } V \in R \\ \emptyset & \text{otherwise.} \end{cases}$$

This prescription uniquely determines the action on arrows. Let $W \leq V$ i.e. $f_{VW}: R(V) \to R(W)$ is a $O(X)^{op}$-arrow. Suppose W is not in R. Then since R is a sieve V is not in R. Hence f_{VW} is defined and in fact is the empty function $\emptyset$. On the other hand, if $W \in R$ then $R(W) \neq \emptyset$ and hence if $V \in R$, $f_{VW}: 1 \to 1$. Otherwise $f_{VW} = \emptyset$.

Notice that the above identification relied on the fact that hom-sets in the category $O(X)$ are either empty, i.e. $hom(V,W) = \emptyset$ iff $W \leq V$; or contain just one arrow, i.e. we can construe them as the terminal object 1. Hence the representable presheaf h_U can be regarded as the functor $h_U(-): O(X)^{op} \to S$ defined by

$$h_U(V) = \begin{cases} 1 & \text{if } V \leq U \\ \emptyset & \text{otherwise} \end{cases}$$

Here again the prescription uniquely determines the action on arrows. Notice that h_U can be identified with the maximal sieve on U. Now given a sieve R on U, for each $V \in O(X)$ we have $R(V) \leq h_U(V)$ and it follows that R is a subpresheaf of h_U. So sieves on U are subpresheaves of h_U. Conversely, subpresheaves of h_U can be identified with a sieve over U. Let F be a subpresheaf of h_U. Define a function $R_F: O(X) \to \{\emptyset, 1\}$ by $R_F(V) = 1$ iff $F(V) \neq \emptyset$. First note that the set $R_F{}^{\bullet} = \{V \in O(X) : R_F(V) = 1\}$ is a collection of subsets of U. We next show that $R_F{}^{\bullet}$ is a sieve on U. Suppose $W \in O(X)$, $W \leq V$ and $V \in R_F{}^{\bullet}$. Since $W \leq V$ there is an $O(X)^{op}$-arrow $f_{VW}: V \to W$. Consider the function $h_U: h_U(V) \to h_U(W)$. F is a presheaf of h_U. So if $h_U(W) = 1$, it must be the case that, given that $F(f_{VW})$ is a restriction of $h_U(f_{VW})$, then $F(W)=1$. But $h_U(W)=1$ if $h_U(V)$ does. We know that $V\in R_F^{\bullet}$ so by definition $R_F(V)=1$ and thus $F(V)=1$. Since $F(V)\leq h_U(V)$ it follows that $h_U(V)=1$. And so $F(W)=1$ and we have $W\in R_F^{\bullet}$.

The next step is to formulate the notion of an F-compatible family for a 'covering' R. A $S^{O(X)^{op}}$-arrow $f:R\to F$ determines for each $V\in R$ (i.e. $R(V)=1$) a function $f_V:1\to F(V)$ such that for $W\leq V$ (hence $R(W)=1$) the the following diagram commutes

$$\begin{tikzcd} 1 \arrow[r, "f_V"] \arrow[d] & F(V) \arrow[d] \\ 1 \arrow[r, "f_W"] & F(W) \end{tikzcd}$$

[diagram 13]

Conversely, any family of such arrows inducing the given commutative property constitute the components of an arrow $f:R\to F$.

Now let $f:R\to F$ be given for a sieve R. The set $\{\sigma_V:\sigma_V=f_V(\emptyset) \text{ and } V\in R\}$ is such that $\sigma_V\in F(V)$. For $V,W\in R$ since $V\Delta W\leq V$ and $V\Delta W\leq W$ we have

$$f_{VW}(\emptyset)=\sigma_{VW}=F_{VW}(\sigma_V)$$

$$f_{VW}(\emptyset)=\sigma_{VW}=F_{VW}(\sigma_W)$$

Thus $\sigma_V{\uparrow_F}V\Delta W=\sigma_W{\uparrow_F}V\Delta W$ and $f:R\to F$ determines an F-compatible family for a sieve R. Conversely, given an F-compatible family for a sieve R we can define a presheaf $f:R\to F$ by defining $f_V:1\to F(V)$ by $f_V(\emptyset)=\sigma_V$ for each $V\in R$. So presheafs of the form $f:R\to F$ can be identified with F-compatible families for R.

Now we may say that a presheaf F is a sheaf if for any $U\in O(X)$, any covering sieve R, i.e. $R\in J(U)$ and any arrow $f:R\to F$ there is a unique element $\sigma\in F(U)$ such that $\sigma\upharpoonright_F V=\sigma_V=f_V(\emptyset)$ for each $V\in R$. Suppose we are given an arrow $f:R\to F$ with $R\in J(U)$ and hence a subpresheaf of h_U. Then we can define an injection from $F(U)$ into arrows of $S^{O(X)^{op}}$ by sending $\sigma\in F(U)$ to the map $g:h_U\to F$ defined by $g_V(\emptyset)=\sigma\upharpoonright_F V$ for each $V\leqslant U$. So the condition that for all $V\in R$, given $\sigma\in F(U)$, we have $\sigma\upharpoonright_F V=\sigma_V$ is that the coresponding g is an extension of f to h_U. Alternatively, that for $V\in R$, the following diagram commutes

$$\begin{tikzcd}
\mathbf{1} \arrow[rd, "f_V"] \arrow[d] & \\
\mathbf{1} \arrow[r, "g_V"'] & F(V)
\end{tikzcd}$$

[diagram 14]

The above analysis of the classical notion of sheaf allows the following reformulation of the original definition:

(*) A presheaf F on a topological space X is a sheaf iff for each $U\in O(X)$ and each covering sieve R on U, any arrow $f:R\to F$ in $S^{O(X)^{op}}$ has a unique extension to an arrow $g:h_U\to F$.

Now we have seen that in the classical case sieves are identifiable with subpresheaves of representable presheaves. This identification also holds if we substitute a category C for $O(X)$. For let R be a sieve on a C-object U. We define the following subpresheaf F_R of h_U. $F_R(V)=\{\alpha:\alpha\in R$ and $\mathrm{dom}(\alpha)=V\}$. For a C-arrow $f:V\to W$ the function $F_R(f):F_R(W)\to F_R(V)$ sends an arrow $\alpha:W\to U$ of $F_R(W)$ to $\alpha\circ f:V\to U$; which since R is a sieve is a member of $R_F(V)$. Conversely given a subpresheaf G of h_U we can derive a sieve from the union of sets {G(V): V a C-object}. Thus by direct analogy this leads to the following generalization of a sheaf for an arbitrary site (C, J):

(**) Let (C, J) be a site, F a presheaf on C, F is said to be a sheaf for a topology J, or a J-sheaf, if for any object U of C and any $R \in J(U)$, each arrow $f: R \to F$ in $S^{C^{op}}$ has a unique extension to an arrow $g: h_U \to F$ in $S^{C^{op}}$.

The category Shv(C, J) takes as object all J-sheaves on C and as arrows all the arrows arising between them in $S^{C^{op}}$. It can be shown that Shv(C, J) is a topos. Categories equivalent to one of this form is called a Grothendieck topos. Clearly, a Grothendieck topos is a "generalized category of sheaves" over a "generalized topological space". We now proceed to explicate Lawvere and Tierney's generalization of this notion.

(ii.b) Lawvere and Tierney's generalization of the notion of sheaf to an arbitrary topos.

Grothendieck had provided a topology for categories $S^{C^{op}}$ where a small category C now replaces O(X), the open sets of a topological space X. Lawvere and Tierney's first task is to provide a generalization of this topology for categories E, where E is an arbitrary topos, now replaces $S^{C^{op}}$. This was achieved through an observed correlation between Grothendieck topologies over $S^{C^{op}}$ and certain characteristic arrows of subobjects of the truth value object Ω in this category.

In the category $S^{C^{op}}$, Ω is a functor $\Omega: C^{op} \to S$. For a C-object U, $\Omega(U) = \{R: R \text{ is a sieve on } U\}$. Let $f: V \to W$ be a C-arrow. Then we have $\Omega(f): \Omega(W) \to \Omega(V)$ defined by $\Omega(f)[R] = f^*(R) = \{\alpha: W \to V \mid f \cdot \alpha \in R\}$. So by (G2) if $R \in J(W)$ then $f^*(R) \in J(V)$ for a Grothendieck topology J. The terminal object 1 is the constant functor $1(U) = \emptyset$ for each C-object U. The arrow $true: 1 \to \Omega$ has components $true_U: \emptyset \to \{\alpha: cod(\alpha) = U\}$. $\Omega(U)$ are the local truth values over U and $true_U$ is the maximal truth value.

A Grothendieck topology J is a function $J: C^{op} \to S$ where J(U) is a family of sieves. Thus $J(U) \leq \Omega(U)$. J is implicitly a functor. For let $f: V \to W$ be a C-arrow. How do we construe $J(f): J(W) \to J(V)$? In the classical case, i.e. where $V \leq W$, for a covering sieve $R \in J(W)$ the natural target for R is the restriction of R to V, i.e. V^*R, which by (C2) is a covering sieve. Thus by extension we make $J(f)[R] = f^*R$ which by (G2) is a member of J(V). Clearly then a Grothendieck topolology is a subpresheaf (subobject) of Ω. So switching attention to the corresponding characteristic arrows Lawvere and Tierney were able to formulate necessary and sufficient conditions reffering to those kinds of entities found in an arbitrary topos. In fact they demonstrated that a subobject of Ω is a Grothendieck topos iff its characteristic arrow $j:\Omega\to\Omega$ satisfies

(LT1) $j\cdot true=j$

(LT2) $j\cdot j=j$

(LT3) $j\cdot \&=\&\cdot(j x j)$

Accordingly then, for an arbitrary topos E, a Lawvere-Tierney topolology is defined as an arrow $j:\Omega\to\Omega$ satisfying (LT1-LT3). In anticipation of the discussion below on 'topos as variable sets' note that Lawvere construed $j:\Omega\to\Omega$ in terms of modality and that it conveyed the idea of a property holding locally. As he puts it in his 1972 "$j:\Omega\to\Omega$ may be thought of as a modal operator to be read "it is j-locally the case that.." [p.9] Goldblatt adds that "The 'local character' of properties of sheaves gives rise to a semantical theory, due to André Joyal, that incorporates aspects of Kripke's [Intuitionist]-semantics, together with the principle that the truth-value of a sentence is determined by its local truth-values." [1979 p.386.] Note that here again is an example of the transition from the local to the global characteristic of sheaves.

A sheaf for a Lawvere-Tierney topology is arrived at by employing a generalization of another notion familiar to topologists, namely: a uniform closure operation. However this new notion differs from a Kuratowski closure operation in that it commutes with finite intersections rather than finite unions. Johnstone takes this point of difference as undermining the sense of 'generalization' inherent in the conditions (LT1)-(LT3) stating that "It is perhaps unfortunate that the word "topology" has survived the process of repeated generalization...for there is very little connection between a topology in the sense of...[Lawvere-Tierney] and a topology on a set." [1977 p.78]

Though, of course, there are no absolute standards to warrant these judgements on generalization, I believe the details in the above development have gone some way in displaying the 'connection'. For example, note that there is an induced natural bijection between Grothendieck topologies on a category C and Lawvere-Tierney topologies in $S^{C^{op}}$. In any case, Bell comments that

..it is a point of genuine historical interest that the characteristic properties of closure operations derived from the notion of *covering* were discovered almost exactly half a century after the isolation of the characteristic properties of closure operations derived from the notion of ***neighbourhood*** [1982 p.333]

A closure operation on an E-object X is a function $K:Sub(X) \to Sub(X)$ satisfying: i) if $Y \leq Y'$ then $K(Y) \leq K(Y')$; ii) $KK(Y)=K(Y)$; iii) $Y \leq K(Y)$. Let $Y \in Sub(X)$, $f:Z \to X$ an E-arrow and $f^*[Y]$ the pullback of Y along f as in the following diagram:

$$\begin{tikzcd} f^*[Y] \arrow[r] \arrow[d] & Y \arrow[d] \\ Z \arrow[r, "f"] & X \end{tikzcd}$$

[diagram 15]

Thus $f^*[Y] \in \mathrm{Sub}(Z)$, i.e. $f^*[Y] \leq Z$. A uniform closure operation on E is an assignment of a closure operation on each E-object X such that for $Y \leq X$ and an arrow $f: Z \to X$ we have that $f^*[K(Y)] = K(f^*[Y])$. Now suppose K is a universal closure condition. So, for example, we can apply K to *true*: $1 \to \Omega$, since *true*$\in \mathrm{Sub}\Omega$. Thus K(*true*)$\in \mathrm{Sub}(\Omega)$. It turns out that the characteristic arrow of K(*true*), i.e. $\chi_{K(true)}: \Omega \to \Omega$, is a Lawvere-Tierney topology. Conversely, a topology $j: \Omega \to \Omega$ may be converted into a uniform closure operation by defining K(Y) to be the subobject classified by $j \cdot \chi_Y: X \to \Omega$ where χ_Y is the characteristic arrow classifying Y. In fact there is a bijection between Lawvere-Tierney topologies and uniform closure operations and characteristically this facilitates the formulation of the desired generalization of sheaf.

First a definition: for a topology $j: \Omega \to \Omega$ and its induced uniform closure operation K a subobject Y of X is 'j-dense' if K(Y)=X. Now in the case of a Grothendieck topology (C,J) on a category $S^{C^{op}}$ in terms of its Lawvere-Tierney topology $j: \Omega \to \Omega$ the following may be demonstrated. Given a monic C-arrow $\sigma: R \to X$ which is j-dense and an arrow $f: R \to F$ if there exists a unique $g: X \to F$ such that $g \cdot \sigma = f$ then F is a J-sheaf. Conversely, this is also a necessary condition on J-sheaf. It is this characterization of a sheaf over a Grothendieck topology that is carried over to an arbitrary topos E.

(#) An E-object X is a j-sheaf iff given a j-dense monic $\sigma: R\to X$ and arrow $fR\to F$, there is a unique $g: X\to F$ such that $g\cdot\sigma=f$.

Let $Sh_j(E)$ be the category with objects the j-sheaves of E and arrows all E-arrows between them. In abstracting the sheaves of E we preserve the essential structure of E, that is, $Sh_j(E)$ is an elementary topos. In this sense we may claim to have constructed an inner model or a site of mathematical activity within the original site. On a formal level we have the straightforward inclusion functor $I: Sh_j(E)\to E$. But the intriguing aspect here is that of the passage from one site to another by way of a topology-induced category of sheaves. How do we analyse this, or more specifically,give a uniform account of this passage? Can we utilise the different topologies to construct sites with certain desired properies? In the categorial context the questions here point to the consideration of a functor from E to $Sh_j(E)$ with certain universal properties. In other words, an adjoint. This functor turns out to be the left adjoint of the inclusion functor I and is known as the 'Sheafification functor'. The functor $S_j: E\to Sh_j(E)$ has the characteristic property that for each E-object X there is an E-arrow $i_X: X\to S_j(X)$ such that, for each j-sheaf Y and arrow $f: X\to Y$, there is a unique arrow $g: S_j(X)\to Y$ such that $f=g\cdot i_X$.

At this point we are in a position to begin to appreciate a most remarkable result. Cohen's forcing technique, a uniform method of controlled passage from one classical site of mathematical activity to another, is in fact a particular case of the more general and insightful process of sheafification. Moreover, in the categorial setting, the full naturality of the forcing technique is appreciated and can be, and in fact has been, further developed with the aid of the powerful heuristic afforded by its analysis in terms of sheafification.

Recall that negation was represented internally by the arrow $\#:\Omega\to\Omega$. Composed with itself this yields the 'double-negation' arrow denoted $\#\#:\Omega\to\Omega$. This is proved to be a Lawvere-Tierney topology and is generally referred to as the double-negation topology. Note that, in propositional logic, for example , a formula α is classically valid iff $\#\#\alpha$ is intuitionistically valid. Furthermore, based on this relationship, the transition of a formula α into $\#\#\alpha$ is a standard means of interpreting classical systems into intuitionistic systems. [See, for example, Gödel's 1933 proof of the consistency of Peano arithmetic relative to its intuitionistic counterpart.] This connection of $\#\#:\Omega\to\Omega$ with intuitionism prompted MacLane to comment of the double-negation topolology that "Thus the topologist Brouwer meets the intuitionist Brouwer." [1979 p.1010] Furthermore the move to double-negation is evident in Cohen's stipulation of the 'weak-forcing' relation - which is clearly analogous to the clauses of Kripke's semantics for intuitionistic logic.

The topology $\#\#:\Omega\to\Omega$ provides us with a sheafification functor $Sh_{\#\#}(-)$ and thus given a topos E the transition is made to $Sh_{\#\#}(E)$. In fact *this is Cohen Forcing!* It is the sheafification functor of this topology that Tierney located as being embedded in Cohen's forcing technique and employed in his categorial analysis and proof of the continuum hypothesis. Moreover, Cohen's forcing technique is a particular case of the general method of shifting from one site of mathematical activity to another by means of a sheafification functor for any given topology.

Following Tierney's categorial proof of the independence of the continuum hypothesis the most notable independence result demonstrated in the categorial context is Bunge's proof of the independence of the Souslin Hypothesis [1974]. But it must be acknowledged that there has been a dearth of classical independence proofs using categorial methods. In this connection at the end of his 1972 Tierney makes the following, somewhat downbeat, statement

In closing, we might make a few remarks as to possible future uses for these sheaf theoretic methods - at least in so far as independence results in logic are concerned. Probably it is fair to say that though one can develop other logical constructions on topos that enable one to establish further classical independence results, for example AC can be handled in this way, it seems unlikely that these methods, using partially ordered sets, will yield many interesting new results in this area - largely because most of them have probably been obtained by more standard techniques.

However, in this treatment we are able to deal with arbitrary categories of forcing conditions, not merely partially ordered sets, and this should prove to be a useful technique in model theory. For example, elementary theories themselves might prove to be interesting sites. Also...most topos are non-classical - in that Ω is not Boolean - and one can make use of this instead of discarding it by passing to ##-sheaves. For example, it seems that the topological interpretation of intuitionism can be thought of simply as mathematics done in Sheaves(T) where T is a topological space. Many independence results in intuitionistic algebra and analysis should be provable by topos methods, though only the surface has been scratched to date.
[1972 pp.40-41]

In the opening of this chapter it was stressed that it was the information on sites of mathematical activity and the transition from site to site that was the centre of foundational interest with respect to Cohen's results. In this case, it is not important that no stream of new classical independence results has been forthcoming using categorial methods. Although, as a matter of fact, Scott was optimistic in this respect, stating "...new models in intuitionistic logic have not as yet resulted in new independence proofs in *classical* set theory. I think we can look forward to some new insights in this direction, nevertheless, when the more abstract models are better understood." [1977 p.xviii] The important point is that we have attained insight into the nature of mathematical activity relative to the variety of sites. Moreover, although Cohen proceeds from classical site to classical site, his construction implicitly contains a passage through an intermediary non-classical site. This point is expanded in the next section. But what the categorial explication of forcing reveals is that mathematical activity is naturally set-theoretically founded but in a denser and richer array of possible universes.

(ii.c) Forcing and Lawvere's notion of 'variable set'.

In this final section I present a brief discussion of Lawvere's notion of 'variable set' focusing on its heuristic for forcing.

The moving force behind the developments within topos theory, especially in connection with foundational and philosophical matters, is William Lawvere. The idea underlying his work in this area is that the shift from ZF set theory to topos theory represents a generalization of set theory but in the specific sense that the former are 'constant sets' whilst a topos is, in general, a universe of 'variable sets'.

Recall that in listing the motivations of his work about the time of the completion of his doctoral dissertation Lawvere mentions the following five developments: Robinson's non-standard analysis; Cohen's independence proofs; Kripke's semantics for the intuitionistic predicate calculus; Giraud's general theory of topoi and his own elementary axioms for the category of abstract sets. Of these developments he states: "...the subsequent unification of which has, I believe, forced upon us the serious consideration of a new concept of set " [1976 p.102] This new notion is that of a 'variable set' - the notion of set inherent in topos theory or, as we have seen, LST. According to Lawvere:

Traditionally, set theory has emphasized the constancy of sets, and both Robinson's nonstandard analysis and Cohen's forcing method involve passing from a system S of supposedly constant sets to a new system S' that still satisfies the basic axioms for constant sets; however, it is striking that both methods pass "incidentally" through systems of variable sets... [1976 p.102]

These systems of variable sets are what I have referred to as the sites implicit in Cohen's forcing technique.

For Lawvere the sites of constant sets are seen as 'limit points' for those of variable sets. He observes that:

Every notion of constancy is relative, being derived perceptually or conceptually as a limiting case of variation and the undisputed value of such notions in clarifying variation is always limited by that origin. This applies in particular to the notion of constant set, and explains why so much of naive set theory carries over in some form into the theory of variable sets. Our inversion of the old theoretical program of modelling variation within eternal constancy has something in common with that of the intuitionists, though we consider variation generally, not only variation of mathematical knowledge; the internal logic of a topos is always concentrated in a Heyting algebra object. [1975 p.136]

In addition, on the theme of the intuitionist connection, Lawvere stated that

I would like to emphasize that recognizing the central importance for mathematics of the Heyting predicate calculus (i.e., intuitionistic logic) in no way depends on accepting a subjective idealist philosophy such as constructivism; objectively variable sets occur (at least implicitly) every day in geometry and physics and the fact that this variation is reflected in our minds in no way means that it is "freely created" by our minds; but it seems to have been the intuitionists who first succeeded in formulating the logic that holds for at least a certain definite portion of variation in general. [1976 p.106]

Now the set ω partially ordered by membership constitutes a category in the usual manner. Consider then the functor category S^{ω}. An object in S^ω is a functor $F:\omega \to S$. Thus such an object is a sequence of sets $X_0, X_1, X_2, \ldots$ with an assignment of a sequence of functions $f_0, f_1, f_2, \ldots$ such that for $n \in \omega$ we have $f_n : X_n \to X_{n+1}$. If the members of ω are construed as moments of discrete time then an object of S^ω are *sets varying over discrete time*. Here ω is referred to as the parameter. In general, we can consider functor categories of the form S^X with an arbitrary parameter, i.e. sets varying over X. These kinds of objects in set-valued functor categories are the paradigms of variable sets.

But in what sense are such objects themselves entitled to be considered as sets? Well, a topos is a generalized set theory so objects in a topos are generalized sets. So the answer to the question is that categories of the form S^X are toposes! The idea can be further generalized by noting that if S is replaced by an arbitrary topos E, the functor category E^X is also a topos. (This latter feature can be taken to be some further confirmation that E is a category of sets.) Furthermore, if functor categories E^X are universes of variable sets then we now have a warrant for the construal of the generalized set theory inherent in topos theory to be the explicit inclusion of the variable sets for which the classical sites are the sets of zero variation i.e. constants. The warrant is that every topos E' is equivalent to one of the form E^X. So toposes are universes of variable sets. Lawvere further explains that

...there has long been in geometry and differential equations the idea that the category of families of spaces smoothly parametrized by a given space X is similar in many respects to the category of spaces itself, and indeed, fromthe point of view of physics, it is perhaps to such a category with X "generic" or unspecified that our stably correct calculations refer, since there are always small variations or further parameters that we have not explicitly taken into account; the "new" concept of set is in reality just the logical extension of this idea.

With reference to the five developments mentioned above Lawvere continues:

... the decisive one for the concept of variable set was the theory of topoi; while nonstandard analysis, the forcing method in set theory, and Kripke semantics all involved...sets varying along a poset X, it was Grothendieck, Giraud, Verdier, Deligne, M. Artin, and Hakim who, by developing topos theory, made the qualitative leap - well-grounded in the developments in complex analysis, algebraic geometry, sheaf theory, and group cohomology during the 1950's - to consideration of sets varying along a small category X and at the same time emphasized that the fundamental object of study is the whole category of sets so varying. Those insisting on formal definitions may thus, in what follows, consider that "variable set" simply means an object in some (elementary) topos (just as, using an effective axiom system to terminologically invert history, we sometimes say that "vector" means an element of some vector space). [1976 p.102. Note here the implicit rejection of the strict view of implicit definition]

Roughly speaking, Cohen's forcing method is a process by which we start with a model of set theory S and then adjoin a 'new' subset of the continuum c to form a model S^* by more or less constructing the closure of $S \cup \{c\}$ under the required set theoretical operations. [See Appendix for the details.] Both S and S^* are classical and if we pick the appropriate c we ensure that S^* has certain elementary properties not satisfied by S, e.g the continuum hypothesis fails in S^*. But as the topos analysis reveals, implicit in this process is the transition of S into a site of variable sets whose variation is then halted at some point of the parameter to form the new site of constant sets S^* with the desired properties. The parameter here corresponds to Cohen's 'forcing conditions'. For example, let P be the set of all finite partial functions ordered by reverse inclusion. P is thus a well-ordered set and as such a category. From S we form the site of variable sets S^{P}. The internal logic here is intuitionistic. The return to constancy is achieved by forming the site $Sh_{**}(S^{P})$.

As Bell informs us:

> It can then be verified that..[$Sh_{**}(S^{P})$]..is Boolean and is in fact categorically *equivalent* to the Boolean extension $V^{(B)}$, where B is the Boolean completion of P. Thus the process of constructing Boolean valued models à la Scott-Solovay amounts to taking double negation sheaves in an intuitionistic model consisting of sets varying over some set P of forcing conditions. [1982 p.334]

But the important foundational point is that the procedure described above employing S and P are only specific examples of a very much more general procedure involving the whole array of sites revealed by topos theory. In these sites mathematical activity is offered a fertile arena for the development and growth of mathematical concepts founded upon a, perhaps still evolving, set theory. The exploration and generation of mathematical concepts within these sites is the shape of mathematical activity to come.

APPENDIX: A GUIDE TO FORCING

BY SAM FENDRICH AND PETER MILNE

THE METAMATHEMATICS

The emergence of formal axiomatic set theories naturally gives rise to questions about the strengths and weaknesses of such systems, particularly if they are to be regarded as foundations of mathematics. Questions as to the power of the continuum and the status of the axiom of choice are important issues in mathematics which have given impetus to the work of set theorists. The system of Zermelo and Fraenkel (ZF) has become the most widely accepted first-order formalization of set theory. The failure to answer these questions within ZF, together with Gödel's proof of the existence of formally undecidable sentences of ZF brought the question of independence to the fore.

A sentence σ is independent of a set of sentences Σ if σ is neither refutable nor provable on the basis of Σ. Thus to show that σ is independent of Σ it suffices to show that $\Sigma \cup \{\sigma\}$ and $\Sigma \cup \{\#\sigma\}$ are both consistent sets of sentences. By Gödel's Completeness Theorem, for first-order theories this is equivalent to the existence of models of $\Sigma \cup \{\sigma\}$ and $\Sigma \cup \{\#\sigma\}$. By Gödel's Incompleteness Theorem, however, we know that in no recursively axiomatizable extention of ZF can we prove the existence of a model of the extended theory. In particular ZF cannot prove its own consistency.

As an illustration of the limitative nature of this result we shall prove a weak form of Gödel's second incompleteness theorem:

THEOREM: If $ZF \vdash \Sigma x(\mathrm{Trans}(x) \,\&\, x \models ZF)$ then ZF is inconsistent.

PROOF: Suppose that $ZF \vdash \Sigma x(\mathrm{Trans}(x) \,\&\, x \models ZF)$ Let x_0 be such an x of least rank. Since $\langle x_0, \epsilon \upharpoonright x_0 X x_0 \rangle \models ZF$ then also $\langle x_0, \epsilon \upharpoonright x_0 X x_0 \rangle \models \Sigma y(\mathrm{Trans}(y) \,\&\, y \models ZF)$. Let $y_0 \epsilon x_0$ be such that $\langle x_0, \epsilon \upharpoonright x_0 X x_0 \rangle \models (\mathrm{Trans}(y_0) \,\&\, y_0 \models ZF)$. Since $\ulcorner \mathrm{Trans}(z) \,\&\, y \models ZF \urcorner$ is a ${}^{ZF}\Delta_1$-formula, it is absolute, and hence $(\mathrm{Trans}(y_0) \,\&\, y_0 \models ZF$ holds. Thus we have shown $ZF \vdash \Pi x((\mathrm{Trans}(x) \,\&\, x \models ZF) \rightarrow \Sigma y \epsilon x(\mathrm{Trans}(y) \,\&\, y \models ZF))$. But x_0 was of least rank and $y_0 \epsilon x_0 \rightarrow \mathrm{rank}(y_0) < \mathrm{rank}(x_0)$ - this is a contradiction. (*)

Clearly, since ZF cannot prove its own consistency, it cannot prove the consistency of any stronger system. We would appear to have reached an impasse in the attempt to prove independence. To get round this Gödel made recourse to the technique of relative consistency proofs, a method first applied by Beltrami and Klein in order to show certain hyperbolic geometries consistent relative to Euclidean geometry. The set of sentences $\Sigma U T$ is consistent relative to the set Σ if, for some formula σ, $(\Sigma U T \vdash \sigma \& \# \sigma) \rightarrow (\Sigma \vdash \sigma \& \# \sigma)$. In model theoretic terms, in a relative consistency proof one shows that if Σ has a model so does $\Sigma U T$. Thus if we can demonstrate that $\Sigma U\{\sigma\}$ and $\Sigma U\{\#\sigma\}$ are both consistent relative to Σ, σ is independent of Σ, provided Σ is consistent. With respect to ZF, Gödel showed that ZF + AC, ZF + CH, and ZF + GCH are all consistent relative to ZF.

In his relative consistency proofs Gödel used inner models. Intuitively one starts by considering the extension of some monadic predicate (in the language of set theory) and shows that this is a proper class "model" of ZF together with some additional axioms. For example, Gödel defined the notion of constructibility in ZF and showed that the constructible sets yield a "model" for ZF+V=L (i.e. ZF+'Every set is constructible'). From ZF+V=L we obtain AC, CH, and GCH. In ZF we cannot, of course, deal directly with proper classes. The intuitive account is a heuristic for the formal procedure outlined in the following theorem:

THEOREM: Suppose that there is a formula $\phi(x)$ with exactly one free variable x such that: (i) $\Sigma \vdash (\Sigma x \phi(x))$; (ii) $\Sigma \vdash \sigma^*$ for all σ in Σ; (iii) $\Sigma \vdash \tau^*$. Then is τ consistent relative to Σ.

PROOF: Suppose $\Sigma U \{\tau\}$ is inconsistent, so there exists $\sigma_1, \ldots \sigma_n$ in Σ such that $(\sigma_1 \& \ldots \& \sigma_n \rightarrow \# \tau)$ is logically valid. Since $\Sigma \vdash (\Sigma x \phi(x))$, we can obtain $\Sigma \vdash (\sigma_1 \& \ldots \& \sigma_n \rightarrow \tau)^*$. [The proof that $\vdash (\Sigma x \phi(x) \rightarrow \alpha^*)$, for any logically valid sentence α, requires a rather tedious induction.] Hence we obtain $\Sigma \vdash (\sigma_1{}^* \& \ldots \& \sigma_n{}^* \rightarrow \# \tau^*)$. So from (ii) we obtain $\Sigma \vdash \# \tau^*$, which contradicts (iii). (*)

Now we know that ZF$\vdash$(V=L$\rightarrow$CH) and so, if we are to use the method of inner models to show the independence of CH, the natural thing to do is to find an inner model in which V=L fails. Unfortunately, as the next theorem shows, this is impossible.

LEMMA. If $A(x)$ is a monadic predicate in the language of set theory such that it defines a transitive inner model A of ZF then $L=L^{A}\subseteq A$. (*)

PROOF OF THEOREM. Suppose A is a transitive inner model of $ZFC+V\neq L$, hence $ZFC\vdash(V\neq L)^{A}$. As L is a model of ZFC, $ZFC\vdash((V\neq L)^{A})^{L}$, i.e. $ZFC\vdash(V\neq L)^{A\Delta L}$. By the lemma $A\Delta L=L$. Hence $ZFC\vdash(V\neq L)^{L}$, which contradicts Gödel's result (assuming ZFC is consistent). The extension of this result to the case where A is a non-transitive inner model follows immediately from Mostowski's Collapsing Lemma applied to proper classes. (*)

Any model of set theory must assign an interpretation to the membership symbol ϵ. In our definition of inner model ϵ is interpreted as the restriction, to the domain of the model, of the membership relation in V (the set-theoretic universe), where the domain is the extension of some monadic predicate in the language of set-theory. A model in which ϵ is so interpreted is termed a standard model. The impossibility result just proved applies only to standard inner models. When we come to Boolean-valued models we can have a non-standard inner model in which $V\neq L$ is true.

Cohen decided to concentrate on finding standard transitive models in which $V=L$ fails. The advantage of considering standard transitive models is that one may utilise the many absoluteness results in determining their properties. For example, the absoluteness of ordinals (i.e. the property of being an ordinal) makes the verification of the axiom of infinity straightforward. In contradistinction to the method of inner models Cohen attempted to "expand the universe".

*

Clearly we cannot hope to start with V and expand outwards, for V contains everything. Our strategy is as follows: as we are carrying out a relative consistency proof we are entitled to assume that there is a model of ZFC, indeed of ZFC+V=L; our decision to work with standard transitive models leads us to the stronger assumption that there is a model of ZFC+V=L of this kind; we expand this model to obtain a model of ZFC+V≠L. [That this is a stronger assumption will be demonstrated below. Although this stronger assumption is not in fact necessary (see below) we make it at this point because by doing so we gain an insight into the heuristics of forcing.] By the strong form of the Downward Löwenheim-Skolem Theorem and Mostowski's Collapsing Lemma we obtain a countable transitive standard model of ZFC+V=L.

We must take as a starting point a countable transitive standard "ground" model because:

THEOREM: From any extension of ZF which is consistent with V=L one cannot prove the existence of an uncountable standard model in which ZF+AC+V≠L obtains.

PROOF: [See Cohen 1966 pp. 108-9]. (*)

This theorem shows that both the ground model and its expansion have to be countable. Further the following theorem tells us why we should expand the ground model to a model containing the same ordinals:

THEOREM: Let α_0 be the least upper bound of the ordinals in the ground model. Hence the ground model has rank α_0 (by absoluteness of the rank function). It is consistent to assume that there is no standard transitive model of rank greater than α_0.

PROOF: If there is no such model we are done. If there is one let α_1 be the least ordinal greater than α_0 for which there is one. By absoluteness considerations any such model of rank α_1 satisfies $\ulcorner(\Pi\alpha>\alpha_0 \# \Sigma x(\mathrm{Trans}(x) \& x \models ZF \& \mathrm{rank}(x)=\alpha))\urcorner$. (*)

Given a ground model **M** the forcing construction provides a method for expanding **M** to countable standard transitive models (generic extensions of **M**) containing exactly the same ordinals. Let **N** be such a generic extension. By absoluteness of ordinals and of constructible sets:

$L^N = \{x \in \mathbf{N} : L(x)\} = \{x : \Sigma\alpha \in \mathbf{N} . L_\alpha(x)\} = \{x : \Sigma\alpha \in \mathbf{M} . L_\alpha(x)\} = L^M \subseteq \mathbf{M} \subseteq \mathbf{N}$.

Clearly where $\mathbf{M} \neq \mathbf{N}$ (and fortunately this is the case in general) **N** will satisfy $V \neq L$.

In the above we have assumed the existence of a standard transitive model of ZFC. We now show that this is rather more than we are entitled to in a relative consistency proof from ZF. Let M be a sentence in the language of set theory stating that ZF has a model, and SM that ZF has a standard transitive model. Given the completeness theorem M is equivalent to CON(ˆZFˆ). To show that not ZF+M$\vdash$SM we use the following result due to Cohen and others [Wilmers and Suzuki, p.15]:

THEOREM: Every standard transitive model of ZF contains an element which is a model of ZF. (*)

Let A be a standard transitive model of least rank. Clearly A$\models$ZF+M+#SM.

Despite this result the Platonist will have no qualms about accepting SM. Platonistically he could "prove" the Reflection Principle for all axioms of ZF simultaneously. This, of course, cannot be formalized within ZF. By the Reflection Principle provable in ZF every finite set of axioms of ZF has a standard transitive model. It would appear that by applying the Completeness Theorem we should obtain a model of ZF. But if we formalize the model theory within ZF we introduce the possibility of non-standard axioms, i.e. axioms corresponding in our coding to non-standard integers, whereas the Reflection Principle holds only for standard axioms. (Our coding may in fact "misbehave" to the extent of giving us non-standard "proofs" of inconsistency. [cf. Drake, p. 96]

Having formalized model theory we obtain the following as a theorem of ZFC: $\Pi x[(\mathrm{Trans}(x) \& x \models ZFC \& |x| = \omega) \rightarrow \Sigma y(x \in y \& \mathrm{Trans}(y) \& y \models ZFC + V \neq L \& |y| = \omega)]$.

The proof of the theorem is facilitated by the forcing construction. We have not yet succeeded in our aim of proving in ZFC that $CON(ZFC) \rightarrow CON(ZFC+V \neq L)$ since we need to adjoin SM to ZFC in order to obtain $\Sigma x(Trans(x) \& x \models ZFC \& |x| = \omega)$.

Obviously we must, in order to achieve our aim, avoid appeal to SM. Shoenfield proposed a formal version of the Platonist's claim in regard to the Reflection Principle. To the language of set theory we add a constant symbol 'c'. We form the theory T by adjoining to ZFC (in the original language) the sentence $\ulcorner Trans(c) \& |c| = \omega \urcorner$, and the relativization to c of every axiom of ZFC.

THEOREM: T is a conservative extension of ZFC.

PROOF: Suppose $T \vdash \sigma$ where σ is in the original language.

So $\vdash (\tau_1 \& \tau_2^{(c)} \& |c| = \omega \& Trans(c)) \rightarrow \sigma$ (where $\tau_1, \tau_2 \in ZFC$)

Thus $\vdash \Pi x((\tau_1 \& \tau_2^{(x)} \& Trans(x) \& |x| = \omega) \rightarrow \sigma)$ and

$\vdash [\Pi x((\tau_1 \& \tau_2^{(x)} \& Trans(x) \& |x| = \omega) \rightarrow \sigma)] \rightarrow [\Sigma x((\tau_1 \& \tau_2^{(x)} \& Trans(x) \& |x| = \omega) \rightarrow \sigma)]$

Hence $\vdash \Sigma x((\tau_1 \& \tau_2^{(x)} \& Trans(x) \& |x| = \omega) \rightarrow \sigma$

Now $ZFC \vdash \tau_1$ and, by the Reflection Principle,

$$ZFC \vdash \Sigma x(\tau_2^{(x)} \& Trans(x) \& |x| = \omega) \rightarrow \sigma$$

Thus $ZFC \vdash \Sigma x((\tau_1 \& \tau_2^{(x)} \& Trans(x) \& |x| = \omega)$

Hence $ZFC \vdash \sigma$ (*)

Shoenfield's approach is perhaps the most elegant in that it permits us to retain the heuristics of the forcing construction, viz. expanding a given model of ZFC, whilst avoiding making recourse to SM.

Cohen's proposal for avoiding SM is that within ZFC we can apply the forcing technique to give us a model of any finite set of axioms of ZFC+V≠L . In order to show that a finite set of axioms Σ of ZFC+V≠L obtain in a generic extension N of a ground model M we require that only a finite set of axioms Σ' of ZFC hold in M. Analysis of the forcing construction will indicate which axioms must belong to Σ'. In ZFC the Reflection Principle guarantees the existence of a model of Σ'. Clearly we lose nothing by taking such a model and expanding it, since all the axioms of ZFC required in the forcing construction to show that Σ holds in the expansion obtain in the model. If not-CON(ZFC+V≠L) then a finite subset of ZFC+V≠L would be inconsistent and have a model in ZFC!

THE HEURISTICS

In this section we give a heuristic account of what we have referred to as "the expansion of the ground model". The ground model is a model of ZF+V=L. The task is to somehow expand the ground model to a model in which V≠L fails, i.e. a model which contains a non-constructible set and satisfies the axioms of ZFC. Mathematically we can view this as the addition of a non-constructible set to the ground model followed by "closing up" under the set-theoretic operations. The problem is that we mean the set-theoretical operations relativized to $M[G]$, i.e. the model we are trying to obtain. The following theorem provides a characterization of a transitive set closed under the set theoretic operations:

THEOREM: A transitive set M which satisfies the following conditions is a model of ZF: (a) $\omega \in M$; (b) every class in M, i.e. every subset of M defined by a formula of ZF relativized to M, which is included in a set in M is itself a set in M; (c) for any formula ϕ whose restriction to M is a functional relation in M, the image under ϕ^M of any set in M which is in the domain of ϕ^M is itself a set in M; (d) for any set $a \in M$, $P(a) \Delta M$ is included in a set in M. (*)

Since ZFC⊬'Every finite set of integers is constructible', the best we can hope for is to find a non-constructible infinite subset of ω. This is readily accomplished by means of a generic filter.

*

We can identify every subset of ω with a function from ω to 2, and conversely. By absoluteness ω and 2 belong to any standard transitive extension of the ground model. As shown above, if the ground model and a generic extension have the same ordinals they have the same constructible sets. If, in such a case, there is a function from ω to 2 in the generic extension which is not in the ground model, then clearly it yields a non-constructible subset of w. If the function, f, is to be non-constructible necessarily it cannot be the characteristic function of any finite set of integers nor of ω itself. Hence $\Pi n\epsilon\omega\Sigma m\epsilon\omega(m>n\&f(m)=1-f(n))$ must be satisfied. Moreover f must also differ from the characteristic function of any subset of ω specificable in advance, i.e., any constructible subset of ω in M, the ground model.

Any function from ω to 2 can be approximated by finite partial functions, i.e. functions into 2 with domain a natural number. If $f\epsilon 2^{\omega}$ then $\Pi n\epsilon\omega f\uparrow n$ is such an approximation ($f\uparrow n\epsilon 2^{<\omega}$). Let $Fn(\omega,2)=2^{<\omega}$ - the set of all finite partial functions from ω to 2. By absoluteness $Fn(\omega,2)\epsilon M$. Furthermore, $Fn(\omega,2)$ can be partially ordered by reverse inclusion and the corresponding partially ordered set $P=\langle Fn(\omega,2),\supseteq\rangle$ belongs to M. It is easy to verify that the union of any filter F over P is a function from a subset of ω into 2. If the function f which we seek is to be the union of a filter F, F must satisfy the following conditions:

(i) Let $Dn=\{p\epsilon Fn(\omega,2):n\epsilon dom(p)\}$, for $n\epsilon\omega$. $\Pi n\epsilon\omega(F\Delta Dn\neq\emptyset)$;

(ii) Let $R_0=\{p\epsilon Fn(\omega,2):0\epsilon Ran(p)\}$, $R_1=\{p\epsilon Fn(\omega,2):1\epsilon ran(p)\}$. $F\Delta R_0\neq\emptyset\neq F\Delta R_1$;

(iii) Let h be a constructible (in **M**) function from ω to 2 and let $Eh=\{p\epsilon Fn(\omega,2):\Sigma n\epsilon\omega(p(n)\neq h(n))\}$. For no such h is $Eh\Delta F=\emptyset$.

The sets Dn, R_0, R_1 and Eh which F is required to intersect have in common the property of being dense in P. In proving that the generic extension of **M** is indeed a model of ZFC, F is required to intersect further dense subsets of P in **M**. One could in principle list all the dense subsets in **M** of P of which F is required to intersect, but in the light of the following theorem this is unnecessary.

THEOREM: If **M** is countable and $p\epsilon Fn(\omega,2)$ then there is a filter F over P, with $p\epsilon F$, which intersects every dense subset of P in **M**. (*)
(Such a filter is said to be P-generic over **M**.)

The last theorem can be generalized to the case where P is an arbitrary partial-order in **M**. However, only if P is atomless can we guarantee that the generic filter does not belong to **M**. This theorem is in turn a special case of the following theorem.

THEOREM: Let P be an arbitrary partial order, $p\epsilon dom(P)$, and $D=\{Dn:n\epsilon\omega\}$ a countable family of dense subsets of P. Then there is a filter F over P such that $p\epsilon F$ and $F\Delta Dn\neq\emptyset$ $(n\epsilon\omega)$. (*)
Henceforth we shall use 'G' to refer to any given P-generic filter over **M**.

THE MECHANICS

In the terms of our heuristic the next step after having obtained a generic filter G on the basis of M and P, is to produce a model of ZFC+V≠L by "closing up" M∪{G} under the set theoretic operations. The problem is to prove the existence of such a model, i.e. to prove the existence of a transitive "generic extension" M[G] of M such that M[G]⊨ZFC, M⊂M[G] and G∈M[G] where M and M[G] have the same ordinals. The way this is achieved in forcing proofs is to "construct" M[G] in such a manner that its properties are completely determined by the properties of M, P and G.

It is at this point that the forcing technique comes into play. And here also is the creative leap in Cohen's work. Since its introduction in his papers of 1963, 1964 and 1966, the technique has been greatly streamlined [cf. Kunen, p.235], sometimes to the point that the definition of the forcing relation emerges for apparently no more reason than that it works. Of course, since forcing *is* a mathematical technique for producing independence proofs, one cannot hope to *derive* it from set theoretic principles, nor do we think that it can emerge in any completely natural manner from a heuristic account, i.e., without, for instance, the appeal to the generalized notions discussed in Part III. We shall, however, given the present framework, sketch the general strategy and then indicate how it is realized.

We know that the domain of the generic extension has to be countable and hence we require only countably many names in order to refer to its members. Let us call such a set of constant terms $T=\langle\tau_n : n\epsilon\omega\rangle$. We can encode into M in the familiar way expressions of the language L(ZF)UT. Forcing is a *relation* which holds between elements of P and encoded *sentences*. If $\phi(x_1,\ldots,x_n)$ is a formula of L(ZF) with just the indicated variables free, and $\tau_1\ldots\tau_n\epsilon T$, then that $p\epsilon P$ forces $\ulcorner\phi(\tau_1\ldots\tau_n)\urcorner$ is written as $p\Vdash\phi(\tau_1\ldots\tau_n)$. The forcing relation is defined by the following:

$p\Vdash\phi(\tau_1\ldots\tau_n)$ iff $\Pi G(G$ is P-generic over M & $p\epsilon G\rightarrow M[G]\models\phi(\tau_1\ldots\tau_n))$. (N.B. where the designata of $\tau_1\ldots\tau_n$) depend essentially on G in a manner to be discussed presently.)

This definition is due to Shoenfield and is a simplification, for expository purposes, of the forcing relation used in carrying through the details of independence proofs.

Since in general not all the generic filters over a partial order P in M belong to M the forcing relation given above cannot be defined in M. But in order to prove for certain axioms of ZFC that they obtain in M[G] we require a modified forcing relation, $\Vdash^*$ ("forcing star"), definable in M. According to Kunen: "There are as many different (equivalent) definitions of $\Vdash^*$ as there are texts on set theory." The important point is that

$$p\Vdash\phi(\tau_1\ldots\tau_n) \text{ iff } p\Vdash^*\phi(\tau_1\ldots\tau_n).$$

The definition of H-* mirrors the semantic definition of truth in a model, though, since in general G is not defined in M, it cannot provide a definition of truth in M[G] for any particular generic filter G. Rather it provides all that is needed for determining the properties of every generic extension simultaneously. In fact we are able to prove the crucial

TRUTH LEMMA: $M[G] \models \phi(\tau_1 \ldots \tau_n)$ iff $\Sigma p \in G(pH\text{-}*\phi(\tau_1 \ldots \tau_n)$

So far we have talked freely about M[G], both as an extension of M which we must prove to a model of ZFC, and as an extant model of ZFC. We have also said that the designata of the names T depend on G and have considered the names in relation to one particular model M[G]. An important aspect of the forcing technique is the relationship between the method of encoding T and the determination of the domain of a generic extension. Diverse methods are used to produce the encoded names (henceforth referred to as elements of the label-space in M) and likewise to produce the domain of M[G]. We shall describe some examples below. First we shall discuss some general considerations which motivate all these methods.

(i) Let L be the label space in M. We can view the domain of M[G] as being the image of L under a bijective function ψ_G, i.e. $\psi_G[L]=\mathrm{dom}M[G]$. As the generic extension is a standard model, $\psi_G(l)\epsilon\psi_G(m) \leftrightarrow M[G] \models (\tau_l \epsilon \tau_m)$, where $l,m\epsilon L$ and l,m are the codes of $\tau_l, \tau_m \epsilon T$. Consider the binary relation induced in L by the ϵ-relation in M[G]: $\{\langle l,m\rangle : \psi_G(l)\epsilon\psi_G(m),\ l,m\epsilon L\}$. Call this relation ϵ_G. Clearly ϵ_G

is a well-founded relation. Thus if we could somehow define ϵ_G independently of M[G], we would obtain M[G] as the Mostowski-collapse of the structure $\langle L, \epsilon_G \rangle$.

(ii) Since $M \subseteq M[G]$ for every generic extension M[G], $\langle \psi_G^{-1}[G], \epsilon_G \rangle$ is a model of set theory isomorphic to M, whose domain is a subset of L. For generic filters G,G' $\langle \psi_G^{-1}[M], \epsilon_G \rangle$ is isomorphic to $\langle \psi_{G'}^{-1}[M], \epsilon_{G'} \rangle$. Simplicity considerations lead us to identify these structures, thus we obtain a subset $L_M \subseteq L$ which serves as a set of standard names for elements of M in every generic extension.

(iii) It is important to note that although the designata of the names T depend on G, the encoding of T into M cannot depend on G. This is because in general it is not the case that $G \epsilon M$ but H^{-*} has to be definable in M.

(iv) As $G \subseteq P$, all its members belong to M, and hence have standard names. The name of G has to be definable in M, and so cannot depend on G itself. We could fix an element Γ of L such that $\Pi G(\psi_G(\Gamma)=G)$. If Γ is chosen so that it is in some way composed from the standard names of all its "potential" members, i.e. those names p' (where p' is the standard name of p) such that $\Sigma G(\psi_G(p')\epsilon G)$, then since $\Pi G(G \subseteq P)$ and $\Pi p \epsilon P \Sigma G(p \epsilon G)$, $\Gamma = \{K(p') : p \epsilon P\}$ where $K(p')$ is some function of p'. Suppose $p \epsilon G$, then $\psi_G^{-1}(p)=p'$, $p' \epsilon_G \Gamma$ and $\Gamma = \psi_G^{-1}(G)$. Hence $\psi_G(p')\epsilon G$. Therefore $G=\psi_G(\Gamma)=\{\psi_G(p') : p \epsilon G\}$. [Notice that $\psi_G(p')$ is independent of G.]

To emphasize the connection with the potential Mostowski collapsing function on the appropriate set, $\langle L, \epsilon_G \rangle$, we can rewrite $G = \psi_G(\Gamma)$ as $\{\psi_G(p'): p' \epsilon_G \Gamma\}$. Ideally we would like a uniform definition of the ψ_G's on L with properties similar to the above particular case. Our first example illustrates how this can be achieved.

EXAMPLE(1) (Cf. Kunen 1980): **The label space is defined by transfinite recursion:** $l \epsilon L$ **iff L is a relation, and** $\Pi \langle m, p \rangle \epsilon l (m \epsilon L \ \& \ p \epsilon P)$. [N.B. The **definition of the label space has P as a parameter and is clearly a definable class in M.**

For each G, P-generic over M, ψ_G **is defined by:**

$$\psi_G(l) = \{\psi_G(m): \Sigma p \epsilon G(\langle m, p \rangle \epsilon l)\}$$

ψ_G **is also defined by transfinite recursion and** $\psi_G[L] = M[G]$.

Let us define the following two functions, where we assume P has a **maximal element** 1_P, **from M into L, by transfinite recursion in M:**

$$m^* = \{\langle n^*, 1_P \rangle : n \epsilon m\} \qquad m' = \{\langle n', p \rangle : n \epsilon m, p \epsilon P\}$$

It is easily shown that $\psi_G(m^*) = \psi_G(m') = m$, **and thus both functions may serve to produce a class of standard names in M.** Clearly in this **example** ψ_G is not a bijection. (Thus in effect, we have more names than we need.) The induced relation ϵ_G is given by:

$$m \epsilon_G \text{ iff } \Sigma p \epsilon G(\langle m, p \rangle \epsilon l)$$

This relation is well-founded and set-like on L (and hence we can obtain the Mostowski-collapse) but since ϵ_G is not extensional the structure $\langle L, \epsilon_G\rangle$ is not isomorphic to M[G].

Taking $\Gamma=\{\langle p', p\rangle : p\epsilon P\}$ then $\Pi G(\psi_G(\Gamma)=G)$.

Without invoking forcing we can show that [G] satisfies the axioms: Extensionality, Foundation, Pairing and Union. H-* is used to obtain Replacement, Power set and the particular axioms under investigation (e.g. $V\neq L$). Given the general forcing technique described above, the problem of producing particular consistency results reduces to the construction of a suitable partial order in **M**.

The following theorem shows the sense in which M[G] is the closure of $\mathbf{M}\cup\{G\}$ under the set theoretic operations.

MINIMALITY THEOREM: $\Pi\mathbf{N}$ if $\mathbf{M}\langle\mathbf{N}$, $G\epsilon\mathbf{N}$ and $\mathbf{N}\models ZFC$ then $M[G]\leqslant\mathbf{N}$. (*)

EXAMPLE(2): (Cf. Cohen 1966.) Cohen's approach is to take the Constructive Hierarchy as primitive rather than the Cumulative Hierarchy. He takes as his ground model the intersection of all countable standard transitive models. By absoluteness of constructibility this is an initial segment of the Constructible Hierarchy, i.e., $\mathbf{M}=\cup\{L_\beta : \beta\langle\alpha\}$ for some countable limit ordinal α. Let $a=ran(\cup G)$ where G is P-generic over **M** and not $G\epsilon\mathbf{M}$. Cohen defines M[G] by "relative constructibility". We know that $a\leqslant\omega$ and a is unbounded in ω hence $TrCl(a)=\omega$. Define $L(0,a)=\omega\cup\{a\}$, $L(\gamma,a)=D(\cup_{\beta<\gamma}L(\beta,a))$ $(\gamma>0)$

where $D(X)$ is the set of subsets of X which are definable by formulae relativized to X using parameters in X. $M[G] = \bigcup_{\beta<\alpha} L(\beta, a)$. The proof that $M[G] \models ZFC$ closely parallels Gödel's proof that the Constructible Hierarchy is a model for set theory. That $M[G]$ satisfies the Minimality Theorem follows immediately from these considerations.

To the language of ZF we add a single new constant $\underline{a}$, which serves as a name for a. To each element of $M[G]$ there corresponds a formula of this extended language and a set of parameters which define that element. Cohen makes use of this fact when he inductively defines the label space. Although Cohen's approach, particularly in regard to the details of the ramified language, lacks the smoothness of development of later accounts, it is more closely related to the heuristics of the forcing technique and is therefore, we feel, worthy of study for other than purely historical reasons.

N.B. When we defined forcing we defined what is sometimes called *weak* forcing. Cohen's original definition is of *strong* forcing ($\Vdash_c$). The two notions are related thus:

$$p \Vdash \phi \leftrightarrow p \Vdash_c \#\#\phi.$$

The logic of strong forcing is intuitionistic, e.g. it is not the case that $p \Vdash_c \#\#\phi \rightarrow p \Vdash_c \phi$. The logic of weak forcing is classical. Kunen attributes the invention of weak forcing to Shoenfield, while Shoenfield credits its introduction to Feferman.

EXAMPLE(3): (Cf. Shoenfield 1971). Shoenfield defines the structure $\langle M, \epsilon_G \rangle$ where M is the domain of our ground model and ϵ_G is defined by:

$$x \epsilon_G y \text{ iff } \Sigma p \epsilon G (\langle x, p \rangle \epsilon y), \text{ for all } x, y \epsilon M.$$

$M[G]$ is the Mostowski-collapse of this structure.

Having first defined the model Shoenfield then introduces the "forcing language". He takes as his label space the whole of M, i.e. $L=M$. This is a technical error, however, for it leaves no room, so to speak, for the encoded formulae of L(ZF)UT, and in consequence forcing is not definable in M. In order to allow the definability of forcing we must restrict L so that we can encode the language.

Shoenfield's approach is instructive because it brings out the fact that $M[G]$ can be constructed independently of L. His mistake (easily rectified) lies in failing to allow for the encoded formulae of L(ZF)UT, not in the fact that he has too many names in his label space. For example Burgess (1977) defines a label space which is a proper extension of Kunen's. Conversely, we may restrict Kunen's definition by requiring that L be a function.

*

With respect to M, but viewed from the outside (since ϵ_G is not definable in M), L is a class with a non-standard ϵ-relation defined on it. The structure $\langle L,\epsilon_G\rangle$ is not a model for set theory as ϵ_G is not extensional. If instead of M we consider the set theoretic universe V (which is what 'M-people' take M to be) and make our definitions accordingly, then $\langle L,\epsilon_G\rangle$ is similar to an inner model with a non standard ϵ-relation. In fact, $\langle L,\epsilon_G\rangle$ satisfies every axiom of ZF other than Extensionality. By taking equivalence classes we can obtain Extensionality: $l\approx m$ iff $\gamma_G(l)=\gamma_G(m)$, $l,m\epsilon L$. Let $L'=\{[l]:l\epsilon L\}$ and define ϵ'_G by $[l]\epsilon'_G[m]$ iff $\gamma_G(l)\epsilon\gamma_G(m)$, $l,m\epsilon L$. The structure $\langle L',\epsilon'_G\rangle$ satisfies $ZF+V\neq L$. Unfortunately the equivalence classes are proper classes. We cannot apply the Mostowski Collapsing Lemma to $\langle L,\epsilon_G\rangle$ in V because ϵ_G is not set-like on L. Alternatively we can obtain Extensionality by defining a new equality relation, $=_G$, on L: $l=_G m$ iff $\gamma_G(l)=\gamma_G(m)$, i.e. iff $l\approx m$. The structure $\langle L,\epsilon_G,=_G\rangle$ satisfies $ZF=V\neq L$.

The definition of $\langle L,\epsilon_G,=_G\rangle$ in V, known as the syntactic model approach, parallels the definition of V^B, the Boolean-valued universe for some complete Boolean algebra B in V. Historically, this latter approach was developed first. The definition of H^- owes much to Shoenfield who realized that one could do the Scott-Solovay construction (Boolean-valued universe) directly with a partial order. Our final section briefly mentions some parallels between forcing as described above and the Boolean-valued model approach to independence proofs.

THE BOOLEAN CONNECTIONS

If, working in V, we define L, the label space, using the restricted version of Kunen's definition, so that every member of L is a function, and further, require that the partial order be a complete Boolean algebra B, then the label space consists of the hereditarily B-valued functions. This class is referred to as the Boolean-valued universe V^B.

By means of a particular inductive mapping, $[.]^B$, every sentence σ in the forcing language is assigned a 'truth-value', $[\sigma]^B$, a member of B. This truth-value definition cannot be given in set theory although the truth-value of any particular σ can be determined within set theory. [Cf Rosser 1969 Chapter 10]. It can be shown that any sentence which is a theorem of ZFC has truth-value 1_B. Thus, if we can find a complete Boolean algebra B such that $0_B<[\sigma]^B<1_B$, then σ is independent of ZFC. Alternatively it may be more practical to find two Boolean algebras, B,B', such that $[\sigma]^B<1_B$ and $[\sigma]^{B'}<1_{B'}$, in order to obtain the independence result.

In order to show that $M[G]\models ZFC$ we needed to introduce $\Vdash^*$. Ostensibly we can show that $V^B\models ZFC$ without recourse to forcing (where $V^B\models\sigma$ iff $[\sigma]^B=1$). However the inductive definition of the map $[.]^B$ mirrors the definition of $\Vdash^*$ and we obtain the following theorem:

THEOREM: $[\sigma]^B = \bigvee\{p\in B: p\Vdash^*\}$ and further, $p\Vdash^*\sigma$ iff $p\leqslant[p]^B$. (*)

In fact if we define $H\!-\!*$ by $pH\!-\!*\sigma$ iff $p\leq[\sigma]^{B}$ then $H\!-\!*$ satisfies the usual forcing conditions.

By selecting the right Boolean algebra B we can show that, say, $V^{B}\models ZFC+V\neq L$. This formal proof of independence nowhere mentions a generic filter over. Analogously, given a partial order P such that there exists a G P-generic over $\mathbf{M}$ such that not $G\in\mathbf{M}$, we could show that $\Sigma p\in P(pH\!-\!*V\neq L)$, without mentioning G or its name Γ. G is, then, only required when producing a transitive standard model of ZFC+V≠L.

In order to construct a Boolean-valued set-model we start, as before, with a ground model $\mathbf{M}$ and the Boolean algebra B such that $B\in\mathbf{M}$ and $\mathbf{M}\models$ ˆB is a complete Boolean algebraˆ. We then relativize our discussion to $\mathbf{M}$ and form L, the label space: $L = V^{B}\Delta\mathbf{M}$. we now have a Boolean-valued universe inside $\mathbf{M}$.

We can prove that for any G P-generic over $\mathbf{M}$, if not $p\in G$ then there exists a $q\in G$ such that p and q are incompatible, i.e. there does not exist an r such that $r\leq p$ and $r\leq q$ $(p,q,r\in P)$. In the case when P is a complete Boolean algebra we show that G is an ultrafilter. Indeed we can prove the following equivalence theorem:

THEOREM: Let $G\leq B$ where B is a complete Boolean algebra in $\mathbf{M}$. G is B-generic over $\mathbf{M}$ if, and only, G is an ultrafilter and the canonocal homomorphism, $h_G:B\to 2$, preserves all suprema in $\mathbf{M}$, i.e. is such that for all $x\in\mathbf{M}$ if $x\leq B$ then $h_G = \vee\{h_G(y):y\in x\}$. (*)

Now, the Rasiowa-Sikorski Theorem tells us that if $\{S_n : n\in\omega\}$ is a countable family of subsets of some Boolean algebra B (not necessarily complete) such that for each $n\in\omega$ the supremum of S_n exists in B, then there is a homomorphism $h: B\to 2$ such that $h(\bigvee S_n) = \bigvee\{h(s) : s\in S_n\}$. Hence the Rasiowa-Sikorski Theorem is the generic filter existence theorem for Boolean-valued models.

The Boolean-valued model approach and the forcing approach both yield the same independence proofs because:

LEMMA: if $i: P\to Q$ is a dense embedding, then P and Q yield the same generic extensions. (*)

THEOREM: Every partial order can be densely embedded in a complete Boolean algebra.

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CORRIGENDA

p. 19, l. 27	:	delete 'it'.
p. 27, l. 3	:	'Zermelo's 1908a'.
p. 35, l. 6	:	'quotation'.
p. 46, l. 7	:	'present description'.
p. 48, l. 20	:	delete sentence beginning 'But I judge .....'.
p. 54, l. 15	:	'fall short'.
p. 58, l. 10	:	'p. 23'.
p. 78, l. 12	:	'Zermelo's 1908a'.
p. 102, l. 23	:	'self-evidence and its necessity'.
p. 127, l. 13	:	'may be'.
p. 183, l. 7	:	delete 'doubt'.
p. 248, l. 23	:	'adjoints etc.'.
p. 248, l. 23	:	'Or, on the other hand, as in L.S.T., we might find versions of the language and theory based on it, giving us ....'.
p. 333, l. 42	:	"Mathematics without foundations".